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ART. IX.—*Researches in Acoustics;* by ALFRED M. MAYER. Paper No. 5,* containing:

1. An Experimental Confirmation of Fourier's Theorem as applied to the decomposition of the vibrations of a composite sonorous wave into its elementary pendulum-vibrations.
2. An Experimental Illustration of Helmholtz's Hypothesis of Audition.
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1. *An Experimental Confirmation of Fourier's Theorem as applied to the decomposition of the vibrations of a composite sonorous wave into its elementary pendulum-vibrations.*

A simple sound is a sound which has only one pitch. Such a sound is produced when, with a bow, we gently vibrate the prongs of a tuning-fork and bring them near a cavity which resounds to the fork's fundamental tone. An almost pure simple sound can be obtained by softly blowing a closed organ-pipe. On examining the nature of the vibratory motions of the

* This paper is the fifth, in the series of those on Acoustics, already published in this Journal. The preceding papers, however, were not numbered.

Sections 1, 2, 3, 5, 6 and 7 of this paper were read before the National Academy of Sciences during the session of November, 1873. Section 4 was read before the Academy on April 21, 1874.

prongs of the fork* and of the molecules of air in the resounding cavity† and in the closed organ-pipe,‡ we find that each of these vibrations follows the same law of reciprocating motion as governs the vibrations of a freely-swinging pendulum. But other bodies, for instance, the free-reeds of organ-pipes and of melodeons,§ vibrate like the pendulum, yet we can decompose the vibrations they produce in the air into many separate pendulum-vibrations, each of which produces in the ear a simple sound of a definite pitch; thus, we see that a pendulum-vibrating body, when placed in certain relations to the air on which it acts, may give rise to highly composite sounds. It is, therefore, evident that we cannot always decide as to the simple or composite character of a vibration reaching the ear solely from the determination of the motion of the body originating the sound, but we are obliged to investigate the character of the molecular motions of the air near the ear, or of the motion of a point on the drum of the ear itself, in order to draw conclusions as to the simple or composite character of the sensation which may be produced by any given vibratory motion. Although we cannot often detect in the ascertained form of an aerial vibration all the elementary pendulum-vibrations, and thus predetermine the composite sensation connected with it; yet, if we find that the aerial vibration is that of a simple pendulum, we may surely decide that we will receive from it only

* In my course of lectures on Acoustics, I thus show to my students that the prong of a tuning-fork vibrates like a pendulum. I take two of Lissajous' reflecting forks, giving, say, the major third interval, and with them I obtain on a screen the curve of this interval in electric light. On a glass plate I have photographed the above curve of the major third passing through a set of rectangular coördinates formed of the sines of two circles whose circumferences are respectively divided into 20 and 25 equal parts. I now place this plate over the condensing-lens of a vertical lantern and obtain on the screen the curve, the circles and their net of coördinates. Suspended over the lantern is a Blackburn's compound pendulum, which is so constructed that its "bob" cannot rotate around its axis. The bob is hollow and a curved pipe leads from its bottom to one side of the pendulum. The pendulum is now deflected into a plane at 45° with its two rectangular planes of vibration so that the end of the curved pipe coincides with the beginning of the curve over the lantern. The bob of the pendulum is fastened with a fine cord in this position and fine hour-glass sand is poured into it; the cord is now burned and the sand is delivered from the pipe, as the swinging pendulum gives the resultant of its motions in the two planes of vibration, while the photographed curve on the lantern is progressively covered with the sand if the tines of the two vibrations of the pendulum are to each other as 4 to 5.

† Helmholtz, Tonempfindungen, 1857, p. 75. Crelle's Jour. für Math. Bd. lvii.

‡ See Mach's Optisch-Akustische Versuche, Prag., 1873, p. 91. Die Stroboskopische Darstellung der Luftschwingungen.

§ The Rev. S. B. Dod, one of the trustees of the Stevens Institute, has recently made an experiment which neatly shows this. He silvered the tips of two melodeon reeds and then vibrated them in planes at right angles to each other, while a beam of light was reflected from them. The resultant figure of their vibrations is the same as that obtained by two Lissajous' forks placed in the same circumstances and having the same musical interval between them as that existing between the reeds.

the sensation of a simple sound. Thus, if we arm the prong of a tuning-fork with a point, and draw this point on a lamp-blackened surface with a uniform motion, and in a direction parallel to the axis of the fork, we shall obtain on the surface a sinusoidal or harmonic curve;* and this curve can only be produced by the prongs of the fork vibrating with the same kind of motion as that of a freely-swinging pendulum. If we now bring this vibrating fork near the mouth of a glass vessel whose mass of air responds to the tone of the fork and, by the method of Mach, examine the vibratory-motions of the air, we shall see it swinging backward and forward; and by combining these vibrations with the rectangular vibrations of forks placed outside of the vessel we shall obtain the curves of Lissajous. If the membrane of the drum of the ear be placed in connection with the resounding cavity, it must necessarily partake of the motion of the air which touches it, and ultimately the auditory nerve fibrillæ are shaken in the same manner, and we receive the sensation† of a simple sound. Here the mind naturally inquires the reason of this connection existing between the sensation of a simple sound and the pendulum-vibration. It has always appeared to me that the explanation of this invariable connection is that the pendulum-vibration is the simplest vibratory motion that the molecules of elastic matter can partake of, and that the connection of the sensation with the mode of vibration is the connection between the simplest sensation perceived through the intervention of the trembling nerves, and the simplest vibration which they can experience. Indeed, the pendulum-vibration is the only one which produces the sensation of sound, for if any other recurring vibration enters the ear it is decomposed by the ear into its elementary pendulum-vibrations, and if it cannot be so decomposed then the given vibration is not recurring and does not produce in us the sensation of sound, but causes that which we denominate as noise. This remarkable connection between a simple sound and the pendulum, or harmonic, vibration, together with the fact of the power of the ear to decompose the motions of a composite sonorous wave into its vibratory elements, was thus distinctly enunciated by Ohm. *The ear has the sensation of a simple sound only when it receives a pendulum-vibration, and it decomposes any other periodic motion of the air into a series of pendulum-vibrations, each of which corresponds to the sensation of a simple sound.*

* The equation of this curve is $y = a \sin\left(\frac{2\pi x}{\lambda} + a\right)$. The length on the axis of one recurring period of the curve is λ; the constant a is the maximum ordinate, or amplitude. The form of the curve is not affected by a, but any change in its value slides the whole curve along the axis of x. It is interesting to observe that this curve expresses the annual variation of temperature in the temperate zones.

† See Helmholtz on the distinction between a sensation and a perception. Tonempfindungen, p. 101.

We have seen that the harmonic curve is the curve which corresponds to the motion which causes the sensation of a simple sound, but a molecule of vibrating air or a point on the tympanic membrane may be actuated by vibratory motions, which, when projected on a surface moving near them, will develop curves which depart greatly from the simplicity of the harmonic, or curve of sines;* but nevertheless these curves will alway be periodic if the sensation corresponding to their generating motions is that of sound. Now Fourier has shown, and states in his theorem, that any periodic curve can always be reproduced by corresponding harmonic curves (often infinite in number) having the same axis as the given curve and having the lengths of their recurring periods as 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c., of the given curve; and the only limitation to its irregularity is that its ordinates must be finite, and that the projection on the axis, of a point moving in the curve, must always progress in the same direction. Fourier demonstrates that the given curve can only be reproduced by one special combination, and shows that by means of definite integrals one can assign the definite sinusoids with their amplitudes and differences of phase. Now Helmholtz† has shown that differences of phase in the constituent elementary sounds do not alter the character of the composite sound, and therefore, that although the forms of the curve corresponding to one and the same composite sound may be infinite in variety (by reason of differences in phase and amplitude in the component curves), yet the composite sound is always resolved into the same elements. This experimental result of Helmholtz also conforms to the theorem of Fourier in reference to the curves projected by such motions; for he has shown that only one series of sinusoidal resolution is possible.

Fourier's theorem can be expressed as follows: The constants C, C_1, C_2, &c., and a_1, a_2, &c., can be determined so that a period of the curve can be defined by the following equation:‡

$$y = C + C_1 \sin\left(\frac{2\pi x}{\lambda} + a_1\right) + C_2 \sin\left(2\,\frac{2\pi x}{\lambda} + a_2\right) + \ldots.$$

But Fourier's theorem is the statement of a mathematical possibility, and it does not necessarily follow that it can be immediately translated into the language of dynamics without experimental confirmation, for, as Helmholtz remarks, "That mode of decomposition of vibratory forms, such as the theorem of Fourier describes and renders possible—is it only a

* In section 5 of this paper, I have constructed several important curves corresponding to composite vibrations.

† Tonempfindungen, p. 190 *et seq.*

‡ For other and more convenient forms of expression of this theorem, as well as for a demonstration of it, see pp. 52 and 60 of Donkin's Acoustics—the most admirable work ever written on the mathematical theory of sound.

mathematical fiction, admirable because it renders computation facile, but not corresponding necessarily to anything in reality? Why consider the pendulum-vibration as the irreducible element of all vibratory motion? We can imagine a whole, divided in a multitude of different ways; in a calculation we may find it convenient to replace the number 12 by 8+4, in order to bring 8 into view; but it does not necessarily follow that 12 should always and necessarily be considered as the sum of 8+4. In other cases it may be more advantageous to consider the number as the sum of 7+5."

The mathematical possibility, established by Fourier, of decomposing any sonorous motion into simple vibrations, cannot authorize us to conclude that this is the only admissible mode of decomposition, if we cannot prove that it has a signification essentially real. The fact, that the ear effects that decomposition, induces one, nevertheless, to believe that this analysis has a signification, independent of all hypothesis, in the exterior world. This opinion is also confirmed precisely by the fact stated above, that this mode of decomposition is more advantageous than any other in mathematical researches. For the methods of demonstration which comport with the intimate nature of things, are naturally those which lead to theoretic results the most convenient and the most clear."

The theorem of Fourier translated into the language of dynamics would read as follows: "*Every periodic vibratory motion can always, and always in one manner, be regarded as the sum of a certain number of pendulum-vibrations.*"

Now we have seen that any periodic vibratory motion, which has the proper velocity, will cause the sensation of a musical note, and that a pendulum-vibration gives the sensation of a simple sound;* therefore, if Fourier's theorem is applicable to the composition and decomposition of a composite sonorous wave, it will be thus related to the phenomena of sound: "*Every vibratory motion in the auditory canal, corresponding to a musical sound, can always, and always in one manner, be considered as the sum of a certain number of pendulum-vibrations, corresponding to the elementary sounds of the given musical note.*"

Heretofore we have called in the aid of the sensations,—assumed to be received through the motions of the co-vibrating

* Professor Donkin, in his Acoustics, Oxford, 1870, p. 11, advises the use of *tone* to designate a simple sound, and the word *note* to distinguish a composite sound. His reasons are "that *tone* (Gr. τόνος) really means *tension*, and the effect of tension is to determine the *pitch* of the sound of a string;" while a musical *note* is generally a composite sound. Professor Donkin further states: "Helmholtz uses the words *klang* and *ton* to signify compound and simple musical sounds. We have followed him in adopting the latter term. But such a sound as that of the human voice could hardly in English be called a *clang*, without doing too much violence to established usage."

parts of the ear,—to help us in our determination of the simple or composite character of a given vibratory motion; but Fourier's theorem does not refer to the subjective effects on the organ of hearing,—the dynamic function of whose parts are yet very imperfectly understood. Ohm's theorem, on the other hand, refers entirely to these subjective phenomena of the ear's analysis of a complex sensation into its simple elements. As Fourier's theorem refers only to the decomposition of a composite recurring vibration into its elementary pendulum-vibrations, it has nothing to do with the physiological fact of the co-relation of the pendulum-vibration and the simplest auditory sensation; though this well ascertained relation gives us the privilege of using this sensation as an indicator of the existence of an aerial pendulum-vibration. Hence, as Fourier's theorem is entirely independent of our sensations, we must endeavor to verify it directly by experiments, which must perform *the actual* decomposition of the composite periodic motion of *a point* into its elementary pendulum-vibrations. But many difficulties present themselves when we would bring to the test of experiment the dynamic signification of Fourier's theorem. For example, the composite sound-vibration, on which we would experiment, emanates from a multitude of vibrating points; parts of the resultant wave surface differ in their amplitudes of vibration; while points equally removed from one and the same point of the body originating the vibrations, may differ in their phases of vibration; so that when such a wave falls upon co-vibrating bodies which present any surface, the effects produced are the results of extremely complex motions. The mind sees at once the difference between this complicated conception and the simple one embodied in the statement of the dynamic application of Fourier's theorem.

As the mathematician decomposes *seriatim* every point of the recurring curve into its harmonic elements, so the physicist, in confirming the dynamic application of Fourier's theorem, should decompose into its simple pendulum-vibrations the composite vibratory motion which such a curve represents, and indeed *reproduces* when it is drawn with a uniform motion under a slit in a diaphragm which exposes to view only a point of the curve at once. Therefore, only one vibrating point of the composite sonorous wave should be experimented on, and the composite vibratory motion of this point should be conveyed along lines to points of elastic bodies which can only partake of simple pendulum-vibrations. All of these essential conditions I have succeeding in securing in the following arrangement of apparatus.

A loose inelastic membrane—(thin morocco-leather does well)—was mounted in a frame and placed near a reed-pipe; or, as in

other experiments, the membrane was placed over an opening in the front of the wooden chamber of a Grenié's free-reed pipe. The ends of several fine fibers from a silk-worm's cocoon were brought neatly together and cemented to one and the same point of the membrane, while the other ends of these fibers were attached to tuning-forks mounted on their resonant boxes, as shown in fig 1. In the experiment which I shall now

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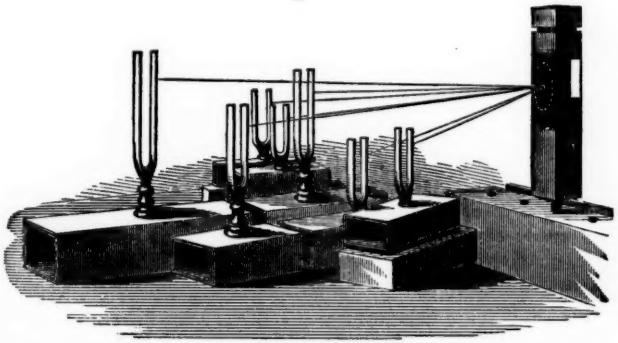


describe, eight forks were thus connected with one point of the membrane. The fundamental tone of the pipe was Ut_2, of 128 vibrations per second; and the pipe was brought into accurate unison with a fork giving this sound.* The forks connected with the membrane were the harmonic series of Ut_2, Ut_3, Sol_3, Ut_4, Mi_4, Sol_4, $B_4^{\flat-}$, Ut_5. In the first stage of the experiment we will suppose that the fibers are but slightly stretched: then, on sounding the pipe, all the fibers at once break up into exquisite combinations of ventral segments. If the sunshine fall upon a vibrating fiber and we look on it obliquely, in the direction of its length, we shall see ventral segments superimposed on ventral segment in beautiful and changing combinations. On gradually tightening the fibers, we diminish the number of their nodes, and on reaching a certain degree of tension with fibers 1 m. long, I have seen them all vibrating with single ventral segments. On increasing the tension, the amplitudes of these single segments gradually diminish and at last disappear entirely, so far as the unaided eye can discern, and then we have reached the conditions required in our experimental confirmation.

* Since the number of beats per second given by any harmonic (of a pipe out of tune with its harmonic series of forks) will be as the order of the harmonic, it is better to tune a reed to unison with a fork giving one of its higher harmonics. I generally used the Sol_3 fork, or the 3d harmonic.

The point of the membrane to which the fibers are attached is actuated by a motion which is the resultant of all of the elementary pendulum-vibrations existing in the composite sonorous wave, and the composite vibrations of this point are sent through each of the fibers to its respective fork. Thus, each fiber transmits to its fork the same composite vibratory motion, while each fork can only vibrate so as to give the simple pendulum-vibration of a simple sound, for each fiber is attached to its fork at a point which lies in the upper node of the segments into which the fork divides when it gives its higher harmonic. Now if Fourier's theorem has "an existence essentially real," any fork will select from the composite vibratory motion, which is transmitted to it, that motion which it has when it freely vibrates; but if its proper vibration does not exist as a component of the resultant motion of the membrane, it will not be in the least affected. Now this is exactly what happens in our experiment, for when the pipe is in tune with the harmonic series of forks, the latter sing out when the membrane is vibrated; but if the forks be even slightly thrown out of tune with the membrane, either by loading them, or by altering the length of the reed, they remain silent when the sounding pipe agitates the membrane and the connecting fibers.* Thus have I shown that the dynamic application of Fourier's theorem has "an existence essentially real."

It is indeed very interesting and instructive thus to observe in one experiment the analysis and synthesis of a composite sound. On sounding the reed it sets in vibration all the forks of the harmonic series of its fundamental note, and after the reed has ceased to sound, the forks continue to vibrate and their elementary simple sounds blend into a note which approximately reproduces the timbre of the reed-pipe. If we could by any means obtain all of the elementary vibrations, and have them with their relative intensities correctly preserved, we should have an echo of the sound of the reed after the latter had ceased to vibrate; but the impossibility of thus obtaining the highest components of the reed, and the difficulty of reproducing the relative intensities of the harmonics in the co-vibrating forks, allow us but partially to accomplish this effect.

2. *An Experimental Illustration of Helmholtz's Hypothesis of Audition.*

The experiment, which we have just described, beautifully illustrates the hypothesis of audition framed by Helmholtz to account for this—among other facts,—that the ear can decompose a composite sound into its sonorous elements. Helmholtz

* See section 4 of this paper for an account of the degree of precision of this method of sonorous analysis.

founds his hypothesis on the supposition that the rods of Corti, in the ductus cochlearis, are bodies which co-vibrate to simple sounds; somewhat, I imagine, as loaded strings * of graded lengths and diameters would act in similar circumstances. The vibrations of the composite wave fall upon the membrane placed near the reed as they fall upon the membrane of the tympanum; and these vibrations are sent through the stretched fibers, (or delicate splints of rye-straw, which I have sometimes used,) from the membrane to the tuned forks, as they are sent from the membrana tympani through the ossicles and fluids of the ear to the rods of Corti. The composite vibration is decomposed into its vibratory elements by the co-vibration of those forks whose vibratory periods exist as elements of the composite wave motion; so the composite sound is decomposed into its sonorous elements by the co-vibrations of the rods of Corti, which are tuned to the elementary sounds which exist in the composite sonorous vibration. The analogy can be carried yet further by placing the forks in line and in order of ascending pitch, and attaching to each fork a sharply-pointed steel filament. If the arm be now stretched near the forks, so that the points of the filaments nearly touch it at points along its length, then any fork will indicate its co-vibration by the fact of its pricking the skin of the arm, and the localization of this pricking will tell us which of the series of forks entered into vibration. The rods of Corti shake the nerve filaments attached to them, and thus specialize the position in the musical scale of the elements of a composite sonorous vibration. Thus a complete analogy is brought into view between our experiment and Helmholtz's comprehensive hypothesis of the mode of audition.

3. *Experiments on the supposed Auditory Apparatus of the Culex Mosquito.*

Ohm states in his proposition that the ear experiences a simple sound only when it receives a pendulum-vibration, and that it decomposes any other periodic motion of the air into a series of pendulum-vibrations, to each of which corresponds the sensation of a simple sound. Helmholtz, fully persuaded of the truth of this proposition, and seeing its intimate connection with the theorem of Fourier, reasoned that there must be a cause for it in the very dynamic constitution of the ear; and the previous discovery by the Marquis of Corti of several thousand† rods of graded sizes in the *ductus cochlearis* indicated

* For discussions of the vibratory phenomena of loaded strings, see Donkin's Acoustics, p. 139; and Helmholtz's Tonempfindungen, p. 267.

† "But all of the propositions on which we have based the theory of consonance and dissonance rests solely on a minute analysis of the sensations of the ear. This analysis could have been made by any cultivated ear, without the aid of

to Helmholtz that these were suitable bodies to effect the decomposition of a composite sonorous wave by their co-vibrating with its simple harmonic elements. This supposed function of the Corti organ gave a rational explanation of the theorem of Ohm, and furnished "a leading thread" which conducted Helmholtz to the discoveries contained in his renowned work, "Die Lehre von den Tonempfindungen."* In this book he first gave the true explanation of timbre, and revealed the hidden cause of musical harmony, which, since the days of Pythagoras, had remained a mystery to musicians and a problem to philosophers.

It may, perhaps, never be possible to bring Helmholtz's hypothesis of the mode of audition in the higher vertebrates to the test of direct observation, from the apparent hopelessness of ever being able to experiment on the functions of the parts of the inner ear of mammalia. The cochlea, tunneled in the hard temporal bone, is necessarily difficult to dissect, and even when a view is obtained of the organ of Corti, its parts are rarely *in situ;* and, moreover, they have already had their natural structure altered by the acid with which the bone has been saturated to render it soft enough for dissection and for the cutting of sections for the microscope.

As we descend in the scale of development, from the higher vertebrates, we observe the parts of the outer and middle ear disappearing, while at the same time we see the inner ear gradually advancing toward the surface of the head. The external ear, the auditory canal, the tympanic membrane, and with the latter the now useless ossicles, have disappeared in the lower vertebrates, and there remains but a rudimentary labyrinth.

Although the homological connections existing between the vertebrates and articulates, even when advocated by naturalists, are certainly admitted to be imperfect, yet we can hardly suppose that the organs of hearing in the articulates will remain stationary or retrograde, but rather that the essential parts of their apparatus of audition, and especially that part which receives the aerial vibrations, will be more exposed than in higher organisms. Indeed, the very minuteness of the greater part of the articulates would indicate this, for a tympanic membrane placed in vibratory communication with a modified

theory, but the leading-thread of theory, and the employment of appropriate means of observation, have facilitated it in an extraordinary degree.

"Above all things I beg the reader to remark that the hypothesis on the co-vibration of the organs of Corti has no immediate relation with the explanation of consonance and dissonance, which rests solely on the facts of observation, on the beats of harmonics and of resultant sounds."—Helmholtz, Tonempfindungen, p. 342.

* According to Waldeyer, there are 6,500 inner and 4,500 outer pillars in the organ of Corti.

labyrinth, or even an auditory capsule with an outer flexible covering, would be useless to the greater number of insects for several reasons; first, such an apparatus, unless occupying a large proportion of the volume of an insect, would not present surface enough for this kind of receptor of vibrations; and secondly, the minuteness of such a membrane would render it impossible to co-vibrate with those sounds which generally occur in nature, and which the insects themselves can produce; similarly, all non-aquatic vertebrates have an inner ear formed so as to bring the aerial vibrations, which strike the tympanic membrane, to bear with the greatest effect on the auditory nerve filaments,* and the minuteness of insects also precludes this condition. Finally, the hard test, characteristic of the articulates, sets aside the idea that they receive the aerial vibrations through the covering of their bodies, like fishes, whose bodies are generally not only larger and far more yielding, but are also immersed in water which transmits vibrations with $4\frac{1}{4}$ times the velocity of the same pulses in air and with a yet greater increase in intensity. For these reasons, I imagine that those articulates which are sensitive to sound, and also emit characteristic sounds, will prove to possess receptors of vibrations external to the general surface of their bodies, and that the proportions and situation of these organs will comport with the physical conditions necessary for them to receive and transmit vibrations to the interior ganglia.

Naturalists, in their surmises as to the positions and forms of the organ of hearing in insects, have rarely kept in view the important consideration of those physical relations which the organ must bear to the aerial vibrations producing sound, and which we have already pointed out. The mere descriptive anatomist of former years could be satisfied with his artistic faculty for the perception of form, but the student of these days can only make progress by constantly studying the close relations which necessarily exist between the minute structure of the organs of an animal and the forces which are acting in the animal, and which traverse the medium in which the animal lives. The want of appreciation of these relations, together with the fact that many naturalists are more desirous to describe many new forms than to ascertain the function of one well known form, which may exist in all animals of a class, has tended to keep many departments of natural history in the condition of mere descriptive science. Those who are not professed naturalists appreciate this perhaps more than the naturalists themselves, who are imbued with that enthusiasm which always comes with the earnest study of any one department of nature; for the perusal of those long and laboriously precise descriptions of forms of organs without the slightest

* See section 4 of this paper.

attempt, or even suggestion, as to their uses, affects a physicist with feelings analogous to those experienced by one who peruses a well classified catalogue descriptive of physical instruments, while of the uses of these instruments he is utterly ignorant.

The following views, taken from the "Anatomy of the Invertebrata by C. Th. v. Siebold," will show how various are the opinions of naturalists as to the location and form of the organs of hearing in the *Insecta*. "There is the same uncertainty concerning the organs of audition (as concerning the olfactory organs). Experience having long shown that most insects perceive sounds, this sense has been located sometimes in this and sometimes in that organ. But in their opinion, it often seems to have been forgotten, or unthought of, that there can be no auditory organ without a special auditory nerve, which connects directly with an acoustic apparatus capable of receiving, conducting and concentrating the sonorous undulations. (The author who has erred most widely in this respect is L. W. Clarke, in Mag. Nat. Hist., Sept., 1838, who has described at the base of the antennæ of *Carabus nemoralis* Illig. an auditive apparatus composed of an *Auricula*, a *Meatus auditorius externus* and *internus*, a *Tympanum* and *Labyrinthus*, of all of which there is not the least trace. The two white convex spots at the base of the antennæ of *Blatta orientalis*, and which Treviranus has described as auditory organs, are, as Burmeister has correctly stated, only rudimentary accessory eyes. Newport and Goureau think that the antennæ serve both as tactile and as auditory organs. But this view is inadmissible, as Erichson has already stated, except in the sense that the antennæ, like all solid bodies, may conduct sonorous vibrations of the air; but, even admitting this view, where is the auditory nerve? for it is not at all supposable that the antennal nerve can serve at the same time the function of two distinct senses.)

"Certain Orthoptera are the only Insecta with which there has been discovered, in these later times, a single organ having the conditions essential to an auditory apparatus. This organ consists, with the Acrididæ, of two fossæ or conchs, surrounded by a projecting horny ring, and at the base of which is attached a membrane resembling a tympanum. On the internal surface of this membrane are two horny processes, to which is attached an extremely delicate vesicle filled with a transparent fluid and representing a membranous labyrinth. This vesicle is in connection with an auditory nerve which arises from the third thoracic ganglion, forms a ganglion on the tympanum, and terminates in the immediate neighborhood of the labyrinth by a collection of cuneiform, staff-like bodies with very finely-pointed extremities (primitive nerve-fibers?), which are sur-

rounded by loosely-aggregated, ganglionic globules. (This organ has been taken for a soniferous apparatus by Latreille. J. Müller was the first who fortunately conceived that with *Gryllus hieroglyphus* this was an auditory organ. He gave, however, the interpretation only as hypothetical; but I have placed it beyond all doubt by careful researches made on *Gomphoceros*, *Oedipoda*, *Podisma*, *Caloptenus* and *Truxalis.*")

"The Locustidæ and Achetidæ have a similar organ, situated in the fore-legs directly below the coxo-tibial articulation. With a part of the Locustidæ (*Meconema*, *Barbitistes*, *Phancroptera*, *Phylloptera*), there is on each side of this point a fossa, while with another portion of this family there are, at this same place, two more or less spacious cavities (auditory capsules) provided with orifices opening forward. These fossæ and these cavities have each, on their internal surface, a long-oval tympanum. The principal trachean trunk of the leg passes between two tympanums, and dilates, at this point, into a vesicle whose upper extremity is in connection with a ganglion of the auditory nerve. This last arises from the first thoracic ganglion, and accompanies the principal nerve of the leg. From this ganglion in question passes off a band of nervous substance, which stretches along the slightly excavated anterior side of the trachean vesicle. Upon this band is situated a row of transparent vesicles containing the same kind of cuneiform, staff-like bodies, mentioned as occurring with the Acrididæ. The two large trachean trunks of the fore-legs open by two wide, infundibuliform orifices on the posterior border of the prothorax, so that here, as with the Acrididæ, a part of this trachean apparatus may be compared to a *Tuba Eustachii.* With the Achetidæ, there is, on the external side of the tibia of the fore-legs, an orifice closed by a white, silvery membrane (tympanum), behind which is an auditory organ like that just described. (With *Acheta achatina* and *italica*, there is a tympanum of the same size, on the internal surface of the legs in question; but it is scarcely observable with *Acheta sylvestris*, *A. domestica* and *A. campestris.*)"

Other naturalists have placed the auditory apparatus of diurnal lepidoptera in their club-shaped antennæ; of bees at the root of their maxillæ; of *Melolontha* in their antennal plates; of *Locusta viridissima* in the membranes which unite the antenna with the head.

I think that Siebold assumes too much when he states that the existence of a tympanic membrane is the only test of the existence of an auditory apparatus. It is true that such a test would apply to the non-aquatic vertebrates, but their homologies do not extend to the articulates; and besides, any physicist can not only conceive of, but can actually construct other

receptors of aerial vibrations, as I will soon show by conclusive experiments. Neither can I agree with him in supposing that the antennæ are only tactile organs, for very often their position and limited motion would exclude them from this function;* and, moreover, it has never been proved that the antennæ, which differ so much in their forms in different insects, are always tactile organs. They may be used as such in some insects; in others, they may be organs of audition; while in other insects they may, as Newport and Goureau surmise, have both functions; for, even granting that Müller's law of the specific energy of the senses extends to the insects, yet the anatomy of their nervous system is not sufficiently known to prevent the supposition that there may be two distinct sets of nerve fibers in the antennæ or in connection with their bases; so that the antennæ may serve both as tactile and as auditory organs; just as the hand, which receives at the same time the impression of the character of the surface of a body and of its temperature; or, like the tongue, which at the same time distinguishes the surface, the form, the temperature and the taste of a body. Finally, I take objection to this statement: "Newport and Goureau think that the antennæ serve both as tactile and auditory organs. But this view is inadmissible, as Erichson has already stated, except in the sense that the antennæ, like all solid bodies, may conduct sonorous vibrations of the air." Here, evidently, Siebold had not in his mind the physical relations which exist between two bodies which give exactly the same number of vibrations; for it is well known that when one of them vibrates, the other will be set into vibration by the impacts sent to it through the intervening air. Thus, if the fibrillæ on the antennæ of an insect should be tuned to the different notes of the sound emitted by the same insect, then when these sounds fell upon the antennal fibrils, the latter would enter into vibration with those notes of the sound to which they were severally tuned; and so it is evident that not only could a properly constructed antenna serve as a receptor of sound, but it would also have a function not possible in a membrane; that is, it would have the power of analyzing a composite sound by the co-vibration of its various fibrillæ to the elementary tones of the sound.

The fact that the existence of such an antenna is not only supposable but even highly probable, taken in connection with an observation I have often made in looking over entomological collections; viz: that fibrillæ on the antennæ of noctural insects are highly developed, while on the antennæ of diurnal insects they are either entirely absent or reduced to mere rudimentary filaments, caused me to entertain the hope that I should

* Indeed, they are often highly developed in themselves while accompanied by *palpi*, which are properly placed, adequately organized and endowed with a range of motion suitable to an organ intended for purposes of touch.

be able to confirm my surmises by actual experiments on the effects of sonorous vibrations on the antennal fibrillæ; also, the well known observations of Hensen encouraged me to seek in aerial insects for phenomena similar to those he had found in the decapod, the *Mysis*, and thus to discover in nature an apparatus whose functions are the counterpart of those of the apparatus with which I gave the experimental confirmation of Fourier's theorem, and similar to the supposed functions of the rods of the organ of Corti.

The beautiful structure of the plumose antennæ of the male *Culex Musquito* is well known to all microscopists; and these organs at once recurred to me as suitable objects on which to begin my experiments. The antennæ of these insects are twelve-jointed and from each joint radiates a whorl of fibrils, and the latter gradually decrease in their lengths as we proceed from those of the second joint from the base of the antenna to those of the second joint from the tip. These fibrils are highly elastic and so slender that their lengths are over three hundred times their diameters. They taper slightly, so that their diameter at the base is to the diameter near the tip as 3 to 2.

I cemented a live male mosquito with shellac to a glass slide and brought to bear on various fibrils a $\frac{1}{5}$th objective. I then sounded successively, near the stage of the microscope, a series of tuning-forks with the openings of their resonant boxes turned toward the fibrils. On my first trials with an Ut_4 fork, of 512 v. per sec., I was delighted with the results of the experiments, for I saw certain of the fibrils enter into vigorous vibration, while others remained comparatively at rest.

The table of experiments which I have given is characteristic of all of the many series which I have made. In the first column (A) I have given the notes of the forks in the French notation, which König stamps upon his forks. In the second (B) are the amplitudes of the vibrations of the end of the fibril in divisions of the micrometer scale; and in column (C) are the values of these divisions in fractions of a millimeter.

A.	B.	C.
Ut_2	·5 div.	·0042 mm.
Ut_3	2·5	·0200
Mi_3	1·75	·0147
Sol_3	2·0	·0168
Ut_4	6·0	·0504
Mi_4	1·5	·0126
Sol_4	1·5	·0126
B^{b}_{4}	1·5	·0126
Ut_5	2·0	·0168

The superior effect of the vibrations of the Ut_4 fork on the fibril is marked, but thinking that the differences in the ob-

served amplitudes of the vibrations might be owing to differences in the intensities of the various sounds, I repeated the experiment, but vibrated the forks which gave the greater amplitudes of co-vibration with the lowest intensities; and although I observed an approach toward equality of amplitude, yet the fibre gave the maximum swings when Ut_4 was sounded, and I was persuaded that this special fibril was tuned to unison with Ut_4 or to some other note within a semitone of it. The differences of amplitude given by Ut_4 and Sol_3 and Mi_4 are considerable, and the table also brings out the interesting observation that the lower (Ut_3) and the higher (Ut_5) harmonics of Ut_4 cause greater amplitudes of vibration than any intermediate notes. As long as a universal method for the determination of the relative intensities of sounds of different pitch remains undiscovered, so long will the science of acoustics remain in its present vague qualitative condition.* Now, not having the means of equalizing the intensities of the vibrations issuing from the various resonant boxes, I adopted the plan of sounding, with a bow, each fork with the greatest intensity I could obtain. I think that it is to be regretted that König did not adhere to the form of fork, with *inclined prongs*, as formerly made by Marloye; for with such forks one can always reproduce the same initial intensity of vibration by separating the prongs by means of the same cylindrical rod which is drawn between them. Experiments similar to those already given revealed a fibril tuned to such perfect unison with Ut_3 that it vibrated through 18 divisions of the micrometer or ·15 mm., while its amplitude of vibration was only 3 div. when Ut_4 was sounded. Other fibrils responded to other notes, so that I infer from my experiments on about a dozen mosquitos that their fibrils are tuned to sounds extending through the middle and next higher octave of the piano.

* I have recently made some experiments in this direction, which show the possibility of eventually being able to express the intensity of an aerial vibration directly in fraction of Joule's Dynamical Unit, by measuring the heat developed in a slip of sheet rubber stretched between the prongs of a fork and enclosed in a compound thermo-battery. The relative intensities of the aerial vibration produced by the fork when engaged in heating the rubber and when the rubber is removed, can be measured by the method I described in the Amer. Jour. Sci., Feb., 1873. Of course if we can determine the amount of heat produced per second by a known fraction of the intensity, we have the amount produced by the vibration with its entire intensity. Then means can be devised by which the aerial vibration produced by this fork can always be reproduced with the same intensity. This intensity, expressed in fraction of Joule's unit, is stamped upon the apparatus, which ever afterward serves as a true measure for obtaining the intensities of the vibrations of all simple sounds having the same pitch as itself. The same operation can be performed on other forks of different pitch, and so a series of intensities of different periods of vibration is obtained expressed in a corresponding series of fractions of Joule's unit. Recent experiments have given $\frac{1}{1000000}$th of a Joule's unit as the approximate dynamic equivalent of ten seconds of aerial vibrations produced by an Ut_3 fork, set in motion by intermittent electro-magnetic action and placed before a resonator.

To subject to a severe test the supposition I now entertained, that the fibrils were tuned to various periods of vibration, I measured with great care the lengths and diameters of two fibrils, one of which vibrated strongly to Ut_3, the other as powerfully to Ut_4; and from these measures I constructed in homogeneous pine wood two gigantic models of the fibrils; the one corresponding to the Ut_3 fibril being about one meter long. After a little practice I succeeded in counting readily the number of vibrations they gave when they were clamped at one end and drawn from a horizontal position. On obtaining the ratio of these numbers, I found that it coincided with the ratio existing between the numbers of vibrations of the forks to which co-vibrated the fibrils of which these pine rods were models.

The consideration of the relations which these slender, tapering, and pointed fibrils must have to the aerial pulses acting on them, led me to discoveries in the physiology of audition which I imagine are entirely new. If a sonorous wave falls upon one of these fibrils so that its wave-front is at right angles to the fibril, and hence the direction of the pulses in the wave are in the direction of the fibril's length, the latter cannot be set in vibration; but if the vibrations in the wave are brought more and more to bear athwart the fibril it will vibrate with amplitudes increasing until it reaches its maximum swing of co-vibration, when the wave-front is parallel to its length and therefore the direction of the impulses on the wave are at right angles to the fibril. These curious surmises I have confirmed by many experiments made in the following manner. A fork which causes a strong co-vibration in a certain fibril is brought near the microscope, so that the axis of the resonant box is perpendicular to the fibril and its opening is toward the microscope. The fibril, in these circumstances, enters into vigorous vibration on sounding the fork; but, on moving the box around the stage of the microscope so that the axis of the box always points toward the fibril, the amplitudes of vibration of the fibril gradually diminish, and when the axis of the box coincides with the length of the fibril, and therefore the sonorous pulses act on the fibril in the direction of its length, the fibril is absolutely stationary and even remains so when the fork, in this position, is brought quite close to the microscope. These observations at once revealed to me a new function of these organs; for if, for the moment, we assume that the antennæ are really the organs which receive aerial vibrations and transmit them to an auditory capsule, or rudimentary labyrinth, then these insects must have the faculty of the perception of the direction sound more highly developed than in any other class of animals. The following experiments will show the force of

this statement and at the same time illustrate the manner in which these insects determine the direction of a sonorous center. I placed under the microscope a live mosquito, and kept my attention fixed upon a fibril which co-vibrated to the sound of a tuning-fork, which an assistant placed in unknown positions around the microscope. I then rotated the stage of the instrument until the fibril ceased to vibrate, and then drew a line on a piece of paper, under the microscope, in the direction of the fibril. On extending this line, I found that it always cut within 5° of the position of the source of the sound. The antennæ of the male mosquito have a range of motion in a horizontal direction, so that the angle included between them can vary considerably inside and outside of 40°,* and I conceive that this is the manner in which these insects during night direct their flight toward the female. The song of the female vibrates the fibrillæ of one of the antennæ more forcibly than those of the other. The insect spreads the angle between his antennæ, and thus, as I have observed, brings the fibrillæ, situate within the angle formed by the antennæ, in a direction approximately parallel to the axis of the body. The mosquito now turns his body in the direction of that antenna whose fibrils are most affected, and thus gives greater intensity to the vibrations of the fibrils of the other antenna. When he has thus brought the vibrations of the antennæ to equality of intensity, he has placed his body in the direction of the radiation of the sound, and he directs his flight accordingly; and from my experiments it would appear that he can thus guide himself to within 5° of the direction of the female.

Some may assume from the fact of the co-vibration of these fibrils to sounds of different pitch, that the mosquito has the power of decomposing the sensation of a composite sound into its simple components, as is done by the higher vertebrates; but I do not hold this view, but believe that the range of co-vibration of the fibrils of the mosquito is to enable it to apprehend the ranging pitch of the sounds of the female. In other words, the want of definite and fixed pitch to the female's song demands for the receiving apparatus of her sounds a corresponding range of co-vibration, so that instead of indicating a high order of auditory development it is really the lowest, except in its power of determining the direction of a sonorous center, in which respect it surpasses by far our own ear.†

* The shafts of the antennæ include an angle of about 40°. The basal fibrils of the antennæ form an angle of about 90°, and the terminal fibrils an angle of about 30°, with the axis of the insect.

† Some physiologists, attempting to explain the function of the semicircular canals, assume, because these canals are in three planes at right angles to each other, that they serve to fix in space a sonorous center, just as the geometrician by his three coördinate planes determines the position of a point in space. But this assumption is fanciful and entirely devoid of reason; for the semicircular canals are always in

The auditory apparatus we have just described does not in the least confirm Helmholtz's hypothesis of the functions of the organ of Corti; for the supposed power of that organ to decompose a sonorous sensation depends upon the existence of an auditory nerve differentiated as highly as the co-vibrating apparatus, and in the case of the mosquito there is no known anatomical basis for such an opinion. In other words, my researches show external co-vibrating organs whose functions replace those of the tympanic membrane and chain of ossicles in receiving and transmitting vibrations; while Helmholtz's discoveries point to the existence of internal co-vibrating organs which have no analogy to those of the mosquito, because the functions of the former are not to receive and transmit vibrations to the sensory apparatus of the ear, but to give the sensation of pitch and to decompose a composite sonorous sensation into its elements; and this they can only do by their connection with a nervous development whose parts are as numerous as those of the co-vibrating mechanism. Now as such a nervous organization does not exist in insects, it follows that neither anatomical nor functional relations exist between the co-vibrating fibrils on the antennæ and the co-vibrating rods in the organ of Corti, and therefore, that neither Hensen's observations on the *Mysis* (assumed by Helmholtz to confirm his hypothesis), nor mine on the mosquito, can be adduced in support of Helmholtz's hypothesis of audition.*

The above described experiments were made with care, and I think that I am authorized to hold the opinion that I have established a physical connection existing between the sounds emitted by the female and the co-vibrations of the antennal fibrillæ of the male mosquito; but only a well established physiological relation between these co-vibrating parts of the animal and the development of its nervous system will authorize us to state that these are really the auditory organs of the insect. At this stage of the investigation I began a search through the zoological journals, and found nearly all that I could desire in a paper, in vol. iii, 1855, of the Quarterly Journal of the Micro-

the same dynamic relation to the tympanic membrane, which receives the vibration to be transmitted always in one way through the ossicles to the inner ear. Really, we determine the direction of a sound by the difference in the intensities of the effects produced in the two ears, and this determination is aided by the form of the outer ear and by the fact that man can turn his head around a vertical axis. Other mammalia, however, have the power of facilitating the determination of motion by moving the axis of their outer ears into different directions. It is also a fact that when one ear is slightly deaf, that the person unconsciously so affected always supposes a sound to come from the side on which is his good ear.

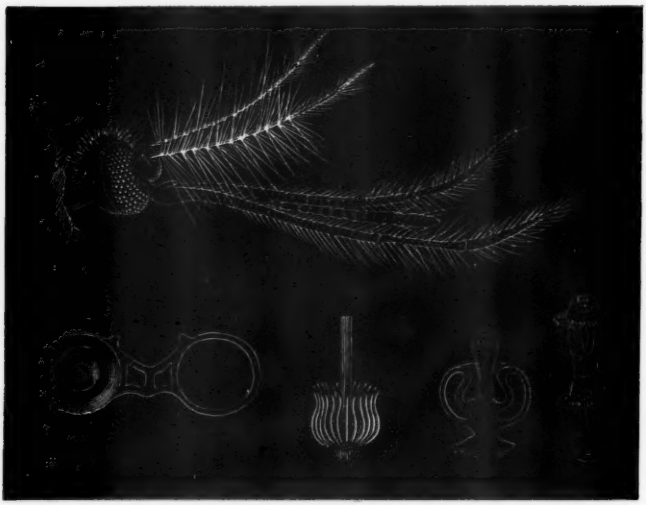
* Also, the organ of Corti having disappeared in the lower vertebrates, it is not likely that it would reappear in the articulata; and especially will this opinion have weight when we consider that the peculiar function of the organ of Corti is the appreciation of those composite sounds, whose signification mammals are constantly called upon to interpret.

scopical Society, entitled "*Auditory Apparatus of the Culex Mosquito*, by Christopher Johnson, M.D., Baltimore, U. S."

In this excellent paper I found clear statements showing that its talented author had surmised the existence of some of the physical facts which my experiments and observations have confirmed. To show that anatomical facts conform to the hypothesis that the antennal fibrils are the auditory organs of the mosquito,* I cannot do better than quote the following from Dr. Johnson's paper:

"While bearing in mind the difference between *feeling a noise* and *perceiving a vibration*, we may safely assume with Carus—for a great number of insects, at least,—that whenever true auditory organs are developed in them, their seat is to be found in the neighborhood of the *antennæ*. That these parts themselves are, in some instances, concerned in collecting and transmitting sonorous vibrations, we hold as established by the observations we have made, particularly upon the *Culex mosquito;* while we believe, as Newport has asserted in general terms, that they serve also as tactile organs.

2.



"The male mosquito differs considerably, as is well known, from the female; his body being smaller and of a darker color,

* A short time before the death of my friend, Prof. Agassiz, he wrote me these words: "I can hardly express my delight at reading your letter. I feel you have hit upon one of the most fertile mines for the elucidation of a problem which to this day is a puzzle to naturalists, the seat of the organ of hearing in Articulates."

and his head furnished with *antennæ* and *palpi* in a state of greater development. (Fig. 2.) Notwithstanding the fitness of his organs for predatory purposes, he is timid, seldom entering dwellings or annoying man, but restricts himself to damp and foul places, especially sinks and privies. The female, on the other hand, gives greater extension to her flight, and attacking our race, is the occasion of no inconsiderable disturbance and vexation during the summer and autumn months.

"The head of the male mosquito, about 0·67 mm. wide, is provided with lunate eyes, between which in front superiorly are found two pyriform capsules nearly touching each other, and having implanted into them the very remarkable antennæ.

"The capsule, measuring about 0·21 mm., is composed of a horny substance, and is attached posteriorly by its pedicle, while anteriorly it rests upon a horny ring, united with its fellow by a transverse fenestrated band, and to which it is joined by a thin elastic membrane. Externally it has a rounded form, but internally it resembles a certain sort of lamp shade with a constriction near its middle; and between this inner cup and outer globe there exists a space, except at the bottom or proximal end, where both are united.

"The antennæ are of nearly equal length in the male and the female.

"In the male, the antenna is about 1·75 mm. in length, and consists of fourteen joints, twelve short and nearly equal, and two long and equal terminal ones, the latter measuring (together) 0·70 mm. Each of the shorter joints has a fenestrated skeleton with an external investment, and terminates simply posteriorly, but is encircled anteriorly with about forty *papillæ*, upon which are implanted long and stiff hairs, the proximal sets being about 0·79 mm. and the distal ones 0·70 mm. in length; and it is beset with minute bristles in front of each whorl.

"The two last joints have each a whorl of about twenty short hairs near the base.

"In the female the joints are nearly equal, number but thirteen, and have each a whorl of about a dozen small hairs around the base. Here, as well as in the male, the parts of the antennæ enjoy a limited motion upon each other, except the basal joint, which, being fixed, moves with the capsule upon which it is implanted.

"The space between the inner and outer walls of the capsule, which we term confidently the auditory capsule,* is filled with a fluid of moderate consistency, opalescent and containing minute spherical corpuscles, and which probably bears the same relation to the nerve as does the lymph in the scalæ of the cochlea of higher animals. The nerve itself, of the antenna, proceeds from the first or cerebral ganglion, advances toward

* See fig. 2.

the pedicle of the capsule in company with the large trachea, which sends its ramifications throughout the entire apparatus, and, penetrating the pedicle, its filaments divide into two portions. The central threads continue forward into the antenna, and are lost there; the peripheral ones, on the contrary, radiate outward in every direction, enter the capsular space, and are lodged there for more than half their length in *sulci* wrought in the inner wall or cup of the capsule.

"In the female the disposition of parts is observed to be nearly the same, excepting that the capsule is smaller, and that the last distal antennal joint is rudimental.

"The proboscis does not differ materially in the two sexes; but the palpi, although consisting in both instances of the same number of pieces, are very unlike. In the female they are extremely short, but in the male attain the length of 2·73 mm.; while the proboscis measures but 2·16 mm. They are curved upward at the extremity.

"* * * The position of the capsules strikes us as extremely favorable for the performance of the function which we assign to them; besides which there present themselves in the same light the anatomical arrangement of the capsules, the disposition and lodgment of the nerves, the fitness of the expanded whorls for receiving, and of the jointed antennæ fixed by the immoveable basal joint for transmitting, vibrations created by sonorous undulations. The intra-capsular fluid is impressed by the shock, the expanded nerve appreciates the effect of the sound, by the quantity of the impression; of the pitch, or quality by the consonance of particular whorls of stiff hairs, according to their lengths; and of the direction in which the undulations travel, by the manner in which they strike upon the antennæ, or may be made to meet either antenna in consequence of an opposite movement of that part.

"That the male should be endowed with superior acuteness of the sense of hearing, appears from the fact, that he must seek the female for sexual union either in the dim twilight or in the dark night, when nothing but her sharp humming noise can serve him as a guide. The necessity for an equal perfection of hearing does not exist in the female; and, accordingly, we find that the organs of the one attain a development which the others never reach. In these views we believe ourselves to be borne out by direct experiment, in connection with which we may allude to the greater difficulty of catching the male mosquito.

"In the course of our observations we have arrived at the conclusion, that the antennæ serve to a considerable extent as organs of touch in the female; for the palpi are extremely short, while the antennæ are very moveable, and nearly equal

the proboscis in length. In the male, however, the length and perfect development of the palpi would lead us to look for the seat of the tactile sense elsewhere, and, in fact, we find the two apical antennal joints to be long, moveable, and comparatively free from hairs; and the relative motion of the remaining joints very much more limited."

My experiments on the mosquito began late in the fall, and therefore I was not able to extend them to other insects. This spring I purpose to resume the research, and will experiment especially on those orthoptera and hemiptera which voluntarily emit distinct and characteristic sounds.

4. *Suggestions as to the function of the Spiral Scalæ of the Cochlea, leading to an Hypothesis of the Mechanism of Audition.*

As the auditory nerve has by far its highest development in the cochlea, it is a natural inference that this part of the ear is chiefly concerned in audition, and that the very peculiar form of the cochlea fulfills some important function; yet the relations of this form to the mode of audition has occupied but little the attention of physiologists. The only suggestion as to the uses of its form with which I am acquainted, is that given by Dr. J. W. Draper in his Physiology, N. Y., 1855. This distinguished scientist states that "it may be imagined how it is that a sound passing through the auditory canal, the bones of the tympanum, the membrane of the fenestra ovalis, and thus affecting its destined portion of the lamina, does not give rise to an idea in the mind of repetition or reverberation by moving back and forth through the two scalæ and affecting its proper nerve fibril at each passage. Is there not a necessity for the exertion of some mechanism of interference which shall destroy the wave after it has once done its work?" Dr. Draper then reasons that this reverberation is prevented by the scalæ being of different lengths and by the fact of their junction in the helicotrema. These two circumstances give rise to interferences, in the helicotrema, of the waves which have proceeded from the stapes up the scala vestibuli with the waves which passed from the membrana tympani across the tympanum to the fenestra rotunda, and thence up the scala tympani to the helicotrema. Dr. Draper also states that when the stapes is pushed in by the contractions of the tensor-tympani and stapedius muscle, the relative length of the scalæ is changed, and thus the proper adjustment for an interference is effected. But even granting that "reverberations or repercussions" take place in a body like the whole apparatus of audition, whose heterogeneous structure must make all of its vibrations, *taken as a mass*, forced oscillations, I do not agree with my distinguished friend in thinking that the difference in the lengths of

the scalæ could bring about any interference except of the most minute and inefficient amount; even if we could agree with Dr. Draper that the intensity of the pulses sent from the fenestra rotunda nearly equal the intensity of those sent up the scala vertibuli from the stapes. The following considerations will make clear our objections to the hypothesis of Dr. Draper. If we take the mean wave-length of the sounds which fall upon the ear as that of the treble G of 440 vibrations per second, it follows that this wave-length will be one meter. But the velocity of sound in the fluid of the scalæ is, at least, $4\frac{1}{4}$ times what it is in air of the same temperature; therefore the average length of the sonorous waves which traverse the scalæ is $4\frac{1}{4}$ meters, and hence for two such waves, meeting in the helicotrema, to completely interfere, one scala would have to exceed the other in length by 2·12 meters. But the entire length of a scala is at the highest only 29 mm., and the difference in their length, taken at its maximum, is so slight that the diminution in the intensity of the resultant wave produced in the helicotrema is inappreciable; and especially will it be so considered when we take into account the relatively feeble intensity of the wave which is sent from the tympanic membrane across the air of the drum on to the membrane of the fenestra rotunda, where two sudden changes in density occur before it passes up the scala tympani.

The following attempt at an explanation of the functions of the spiral stairways of the cochlea is given merely as a suggestion, and with the hope that I may thereby call the attention of students of physiological acoustics to the consideration of the uses of these peculiar forms. Recent studies in embryology and comparative anatomy have shown that the ductus cochlearis is the essential part of the ear, and that the forms of the scalæ are determined by it; for "the original soft parts of the cochlea are distinct from their osseous capsule, which belongs to the petrous bone; the scalæ are secondary formations around the principal canal of the cochlea, the ductus cochlearis, whose epithelial lining proves eventually to be the germ center, so to speak, of the entire apparatus." (*Waldeyer, On the Auditory Nerve and Cochlea;* in Stricker's Histology.) The fact that the ductus controls the form of the scalæ, and not *vice versa*, shows that the scalæ must bear some very important functional relation to the ductus. This relation will become evident on considering the actions which must take place when a sound-wave traverses the scalæ.

All know that the organ of Corti is enclosed in the ductus cochlearis, a canal of triangular section bounded on two of its sides by the scalæ, and on its third by the membranes lining the outer wall of the cochlea. The upper wall of this canal is

formed by the membrana Reissneri, which separates it from the scala vestibuli, and its lower wall is the lamina spiralis, and the elastic membrana basilaris, which separate it from the scala tympani. The ductus is closed at its upper end, and at its lower end it communicates with the sacculus hemisphericus by a fine duct. The arch of Corti rests upon the membrana basilaris, which extends beyond the base of the arch to the membranous outer wall of the cochlea, and over the arch spreads the membrana tectoria, covering the rods of Corti and the hair-cell cords as with a roof, but leaving the outer portion of the elastic membrana basilaris exposed. We will now show that the significance of these anatomical relations is to bring the sound vibrations to act with the greatest advantage on the co-vibrating parts of the ear, and to cause these parts to make one-half as many vibrations in a given time as the tympanic or basilar membranes.

The relations which the form of the scalæ bears to the sonorous waves traversing them, will be modified according to the existence or non-existence of a communication between the scalæ. On this point there seems to be some difference of opinion, and, therefore, I will attempt to explain the functions of the scalæ, first, on the supposition that they are continuous, and then on the assumption that they are not continuous, but closed at the place where the helicotrema is supposed by most anatomists to exist.

E. Weber was the first to point out the peculiar molecular actions which exist when the dimensions of a body are very small compared with the length of the sonorous waves which traverse it; and Helmholtz based his investigations on *the Mechanism of the Ossicles of the Ear* on the theory of Weber, which Helmholtz gives in these words: "The difference in displacement of two oscillating particles, whose distance from one another is infinitely small compared with the wave-length, is itself infinitely small compared with the entire amplitude of displacement." It is evident that the compressions and dilatations which may exist in any body, depend entirely on the differences in the phases of the vibrations constituting the sonorous wave, and when the body has a depth equal to half a wave-length it can embrace the maximum amount of condensation and rarefaction. But condensation and dilatation alone produce *lateral action* on the walls of a straight canal traversed by sonorous vibrations, and hence, if the length of the canal be but a small fraction of the wave, then there exists throughout the canal but little difference in phase, and therefore but little lateral action. Now the united lengths of the scalæ is but a small fraction of the mean length of the sonorous waves which traverse it; for if we take, as above, $4\frac{1}{2}$ meters as the mean

length of the waves which are propagated through the scalæ, and 59 mm. as the length of the united scalæ, it follows that the latter is only $\frac{1}{7\frac{1}{2}}$ of the mean wave-length. Now if we imagine the scalæ straightened and forming one continuous tube with a free communication existing at the helicotrema, then the mean wave traversing them will cause only $\frac{1}{2\frac{1}{2}}$ of the lateral action which this same wave would produce if the scalæ had the length of half of the wave, and it follows that the whole liquid of the scalæ would vibrate forward and backward almost as an incompressible mass, approaching in character to the oscillations of a solid piston in a cylinder; therefore, the action against the walls of the ductus cochlearis would be very slight. But now consider the change in effect on the ductus which takes place when it, together with the scalæ, is wound up into such an ascending spiral as exists in the ear. The molecules of the liquid in the scalæ, thrown forward and backward by the vibrations of the stapes, tend to move in straight lines, but the now curved form of the scalæ causes them to press against the outer or peripheral part of the upper wall (membrana Reissneri) of the ductus cochlearis, and against the outer part of the lower wall (membrana basilaris) when the stapes moves inward, and when it moves outward this action of compression is relieved from the two opposite walls of the ductus. But these actions produced by the stapes on the two walls of the ductus are opposed to each other, and since they take place simultaneously and with about the same intensity, (by reason of our assumption of the free communication of the scalæ,) the rods of Corti and the hair-cells will not vibrate but will only experience compressions and dilatations like the fluid in which they are immersed. Therefore, there appears to me a physical basis for the opinion that either there is no communication between the scalæ, or, if the helicotrema exist, that it must be a very constricted passage. Indeed, if we adopt the latter view, then everything works to produce the maximum effect on the co-vibrating parts of the organ of Corti; for when the stapes moves inward the pressure is thrown on the outer border of the upper wall, or roof, of the ductus, thence across to the peripheral portion of the basilar membrane. This action, we may say, takes place simultaneously throughout the whole length of the ductus, moves downward the floor of the basilar membrane, and thus presses the fluid of the scala tympani against the fenestra rotunda and moves this membrane outward. When, however, the stapes moves outward, the pressure is relieved from the elastic basilar membrane, which is now moved upward, while the fenestra rotunda moves inward.*

* If we could examine, at the same time, vibrating points on the stapes and on the fenestra rotunda with a vibration-microscope, I imagine that these points would exhibit no difference in phase when the membrana tympani vibrated to a note below the treble.

There are also other anatomical facts besides the inclination of the membrana Reissneri to the plane of the membrana basilaris, and the inclination of both these membranes to the plane perpendicular to the axis of the cochlea, which favor an opinion that the outer or peripheral part of the basilar membrane receives the main part of the vibrations which enter the ductus. The auditory nerve fibrils are not attached to the Corti rods or pillars, as was formerly imagined; and, therefore, these bodies cannot be the co-vibrating parts of the ductus; but the Corti pillars appear to act, in conjunction with the cylindrical nerve-cells of Hensen, as supports for the lamina reticularis, between which and the basilar membrane are steadily and tensely stretched the hair-cell *cords* (as I will term them); and to these cords are attached the nerve-fibrils. Waldeyer says, on this point, that "The outer radial fibers direct their course, as Gottstein has found, toward the tunnel of Corti, passing between the inner pillars and traversing the tunnel about midway between the summit and base of the arch; in a profile view these fibers appear like stretched harp-springs. On leaving the arched space they pass between the outer pillars and direct their course —rising a little toward the scala vestibuli—straight to the hair-cells, with which they become completely fused. In several preparations from the dog and the bat I have seen this termination of the nerves in the most convincing manner, at least so far as the innermost row of hair cells is concerned; as to the other rows, we may pretty confidently assert that the termination of the nerves is the same, for we can frequently see several fibers passing at the same time between the outer pillars." The very fact that the number of these hair-cell cords increases with the higher development of the ear shows their important function; for, while in man they are arranged alternately in five rows and number 18,000, in other Mammalia there are only two or three rows.* These hair-cell cords are more perpendicular to the basilar membrane than the Corti rods, and are also different in their forms, having swellings in the middle of their lengths. These swellings must cause them to act like loaded strings, and each hair-cell cord is peculiarly well adapted to co-vibrate with only one special sound. Also, these hair-cell cords are placed in reference to the sound pulses, striking them somewhat in the relation which the antennal fibrils of the mosquito bear to a wave-surface to which their lengths are per-

* It is to be regretted that no accurate measures of the lengths and diameters of the rods and cords of the organ of Corti have been secured. The outer pillars of the arch of Corti certainly double their length in going from the base to the top of the ductus; but does this fact point them out as bodies suitably proportioned to co-vibrate to sounds extending through at least *eight octaves?* I known of no measures on the hair-cell cords. When their dimensions are determined, physiologists will be able to give more precision to their hypotheses.

pendicular. The hair-cell cords, therefore, will not be set in vibration by the action of the feeble pulses which may reach them directly through the membrana Reissneri from the scala vestibuli; and furthermore, the shielding influence of the membrana tectoria tends to prevent this direct action on the cords.

If my view be correct, that these cords receive their vibrations from the basilar membrane, and not directly from the impulses sent into the ductus, it necessarily follows that these cords bear to the membrane, to which they are attached, the same relation as stretched strings bear to the vibrating tuning-forks in Melde's experiments; and, therefore, *a cord in the ductus will vibrate only half as often in a second as the basilar membrane to which it is fastened.* Experiments, similar to those described in section 1 of this paper, illustrate very well our hypothesis of audition. Thus, the membrane, placed near the sounding reed, stands for the basilar membrane; strings, of various lengths and diameters and loaded at their centers, are fastened to the membrane and represent the hair-cell cords. On sounding the reed-pipe, only those strings in tune with the harmonics existing in the composite sound of the reed will enter into vibration; just as when the same sound vibrations enter the ear, and vibrate the basilar membrane, the only hair-cell cords which enter into vibration are those in tune with the elementary vibrations existing in the membrane. Also, it is to be observed that as the loaded string makes one vibration to two of the membrane, so the hair-cell cord makes only one vibration to two of the basilar membrane.

If it be true that when simple vibrations impinge on the ear, the tympanic and basilar membranes vibrate twice, while the co-vibrating body only vibrates once, then it follows that if the same simple vibrations can be sent directly to the co-vibrating parts of the ear, without the intervention of the basilar membrane, we should perceive a sound which is the octave of the one we experienced when the same simple vibrations entered the ear through the tympanic membrane. Hence it appears that our hypothesis can be brought to the test of experiment in the following manner: A tuning-fork held near the ear causes a sensation corresponding to the designated pitch of the fork. But the vibrations of this fork can be sent to the inner ear through the bones of the head; and although we cannot prevent the simultaneous vibration of the tympanic and basilar membranes, yet we can at the same time directly vibrate all the parts of the inner ear. Therefore, if we first hold this fork near the ear and note its pitch and the quality of its sound, and then press its foot firmly against the temporal bone, we should perceive a marked difference in the timbre of the fork when sounded in these two different positions; for when its foot is

against the head we should hear the usual simple sound of the fork accompanied by its octave.

Thus, if we take an Ut_8 fork and vibrate it near the ear, and closely apprehend the character of its sound, we shall experience a sensation which certainly does not contain that corresponding to the higher octave of the fork. Now, press firmly the foot of the fork against the zygomatic process, close to the ear, directing the foot of the fork somewhat backward, and we shall distinctly hear the higher octave of the fork singing in concert with its real note. If the auditory canal be now closed by gently placing the tip of the finger over it, we shall perceive the higher octave with an intensity almost equal to that of the fundamental note. The same sensation, though less intense, may be obtained by placing the fork on any part of the temporal bone. One can also perceive distinctly the higher octave when the fork is placed on the parietal bone, about two inches in front of and an inch or so to the side of the foramen, and its foot directed toward the opposite inner ear, while the auditory canal of this ear is gently closed with the tip of the finger. But the higher octave sings out with the greatest intensity when the foot of the fork is placed on the tragus of the outer ear. A friend, who is a musician as well as a physicist, repeated these experiments, and he informs me that when the foot of the fork is placed against the tragus of his ear he hears the higher octave to the almost entire exclusion of the lower, and with a clearness that reminds him of the sensation perceived when an Ut_4 resonator, placed to the ear, reinforces its proper note. The higher octaves of several forks have been thus perceived, but the forks from Ut_3 to Ut_4 inclusive appear to give the best results.

The fact that sound pulses sent to the inner ear through the head give the sensation corresponding to the higher octave of that perceived when the fork vibrates the air outside the ear, and, therefore, that different co-vibrating parts of the ear are set in action by the vibrations reaching the ear by these two different routes, is a necessary consequence of my hypothesis of the mode of audition, and was not suspected until my hypothesis pointed it out to me, and was not known until I attempted to test the hypothesis by experiment. I know of no other hypothesis that accounts for this fact, which, while it is a necessary consequence of my own views, is directly opposed to those hypotheses hitherto formed on the mode of audition; for, according to the latter, the co-vibrating parts of the ear make as many oscillations in a given interval as the tympanic and basilar membranes.

[To be continued.]

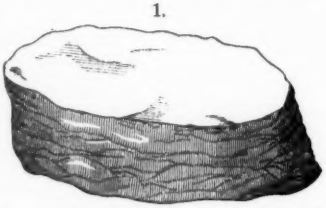
Art. X.—*On the so-called Land Plants from the Lower Silurian of Ohio;* by J. S. Newberry.

[Read before the National Academy of Sciences, at the meeting in April, 1874.]

In the January number of this Journal, Mr. Leo Lesquereux describes two fossils, found in the upper portion of the Cincinnati group, near Lebanon, Ohio. These he considers as the remains of land plants, and refers them to the genus *Sigillaria;* and this case is cited as the first instance where plants so highly organized have been met with in Lower Silurian rocks. Through the kindness of the Rev. H. Hertzer, to whom the specimens in question belong, they had been in my possession some time before the publication of Mr. Lesquereux's notice, and I had examined them with some care for the purpose of determining, if possible, their botanical relations. I had also made careful drawings of them, of which copies are herewith submitted. As the result of my examination, I am compelled to say that I fail to find, either in the external characters or internal structure of these specimens, any satisfactory evidence that they represent land plants; still less that they form species of the genus *Sigillaria.* Their external markings are fairly represented in the accompanying figures, and they exhibit no internal organic structure whatever. They are simply casts in earthy limestone without carbonaceous matter, or any traces of woody tissue.

The smaller specimen (fig. 1) is a discoid section of a cylindrical trunk, of which the external surface is very smooth, but is marked hy a reticulation not unlike that of one section of the genus *Sigillaria.* I did not discover, however, any dots or tubercles in the center of the meshes, such as are referred to by Mr. Lesquereux, and which, were they present, might be supposed to represent the place of the nutrient vessels of the leaves. Taken by itself, I should say that this specimen might be a sponge or some other low form of marine life, quite as well as a *Sigillaria.* Since it is so small and forms so little of the original organism, I think it would be unsafe to make it the base of any general and important conclusion.

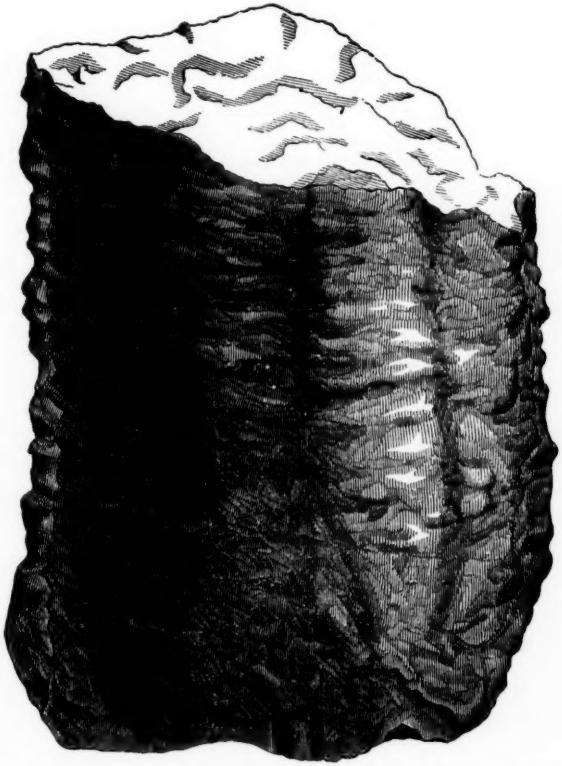
1.



The larger specimen (fig. 2) is represented, like the other, of natural size. This is also a cast of a nearly cylindrical trunk, of which the external surface is roughened by irregularly disposed and unequally sized lenticular prominences. These resemble, in a rude way, the leaf scars borne by the trunks of

some Lycopodiaceous or Cycadaceous plants, but they do not exhibit the spiral arrangement, nor the details of structure

2.



which the leaf-scars of such plants almost uniformly retain in the fossil state. In the interior of this trunk are seen a few of the irregularly scattered points of carbonaceous matter, but they are not continuous fibers, and to my eye show no traces of cell structure.

Taking all the characters of these interesting fossils into consideration, I am disposed to regard them as casts of the stems of fucoids. Had they been land plants, they would almost certainly exhibit more distinctness and regularity of surface-marking, some coating of carbonaceous matter, and some traces of organic structure. A large number of specimens of sea-floated land plants, which we have found in the Devonian limestones of Ohio, all assert their botanical affinities by these characters. The remains of fucoids, on the contrary, consist almost

universally of mere casts of their external surface, carbonaceous matter and internal structure having both entirely disappeared.

The physical condition of the region about Cincinnati, during the Lower Silurian age, strengthens the conclusion that the specimens under consideration are not the remains of land plants. As I have shown elsewhere,* the Cincinnati arch was raised at the close of the Lower Silurian age. Subsequent to that time it formed a group of islands, which, during the Devonian age, were probably covered with a luxuriant terrestrial vegetation. But during the period when the Cincinnati group was deposited an open sea occupied all this part of the Mississippi Valley. The shores of this sea were formed by the Blue Ridge, the Adirondacks, the Canadian Highlands and the Eozoic area on the south shore of Lake Superior, nowhere nearer than 500 miles from the locality where these specimens were found. In these circumstances we must regard it as extremely improbable that specimens of two species of land plants should be floated from this far-off shore and should be deposited together in the calcareous sediment accumulating at the sea bottom near where Cincinnati now stands. That fucoids should be found there is, however, not at all strange, for they float to all parts of all oceans, and other fucoids are frequently met with in the Cincinnati group of this vicinity.

For the reasons given above, I should hesitate to rest upon these specimens so important a conclusion as that promulgated by Mr. Lesquereux. I would not be understood, however, to assert positively that they are *not* the remains of land plants, for they are too imperfect to be decisive of that question, but only this, that they do not afford characters which permit me to accept them as evidence of the existence of land plants, and certainly not of *Sigillariæ* in Ohio, during the Lower Silurian age.

The remains of what have been called land plants have been discovered in the Lower Cambrian sandstones of Sweden, and two species of these have been described (*Eophyton Linneanum* Torell, and *E. Torelli* Linnarsson). The specimens are said by algologists not to be the remains of algæ, but they are considered to be vascular cryptogams or monocotyledons. It is not certain, however, that they are not thallogens, as all traces of structure are lost and nothing is left but the impression of the external surface.†

The evidence of the existence of land plants during the Upper Silurian age is more satisfactory. Prof. J. W. Dawson of Montreal has announced the discovery of vascular cryptogams

* Geological Survey of Ohio, vol. i, part i, page 93.

† Geological Magazine, Sept., 1869. [In the Ofversigt af Kongl. Vetenskaps-Akademiens Förhandlingar, 1873, No. 9, Stockholm (the Bulletin of the Royal Swedish Academy) A. Nathorst has an article, illustrated by several plates, is which he shows a close resemblance between the forms of Eophyton and the trailn of drifting Fuci and other plants, and suggests this origin for them.—Eds.]

in the Upper Silurian strata of Gaspé, Canada.* Here, with a large number of fucoids, a few specimens have been found, which he refers to his genus *Psilophyton*. In these the scalariform axis and the outer fibrous bark both remain, and serve as guides in their classification.

With these exceptions, no land plants are reported below the Devonian. On this point, however, the evidence is all negative, and highly organized land plants may be at any time found in the Lower Silurian rocks. Indeed, the variety and high rank of the Devonian flora prepares us to expect such a result. Strict accuracy compels us to state, however, that up to the present time positive proof of the existence of land plants in the Lower Silurian has not been met with in other countries, nor is it furnished by the specimens under consideration.

Art. XI.—*A Criticism upon the Contractional Hypothesis;* by Captain C. E. Dutton, U. S. A.†

The hypotheses, which have been put forth to explain the part performed by hypogeal forces in the evolution of the surface features of the earth, are here referred to two types: 1st, those attributing them to the contraction resulting from secular loss of heat: this for the sake of convenience will here be called the *contractional hypothesis;* 2d, those arguments which have been resorted to in order to explain isolated facts, or groups of facts, by attributing them to the reaction of the interior to disturbances produced by external changes: this will be called the *reactional hypothesis.*

The argument for the contractional hypothesis presupposes that the earth-mass may be considered as consisting of two portions, a cooled exterior of undetermined (though probably comparatively small) depth, inclosing a hot nucleus. Although of some importance to the argument, it is not regarded as a vital question whether the nucleus be solid or fluid, nor whether the two portions be abruptly distinguished, or merely differentiated from each other. The secular loss of heat, it is assumed, would be greater from the hot nucleus than from the exterior, and the greater consequent contraction of the nucleus would therefore gradually withdraw the support of the exterior, which would collapse. The resulting strains upon the exterior would be mainly tangential. Owing to considerable inequalities in the ability of different portions to resist the strains thus devel-

* Dawson, Precarboniferous Plants of Canada, p. 66.

† This paper is one of several communications to the Washington Philosophical Society made during the winter of 1872–73, and has not hitherto been published except by title and brief abstract in the minutes of that society.

oped, the yielding would take place at the lines, or regions of least resistance, and the effects of the yielding would be manifested chiefly, or wholly, at those places, in the form of mountain chains, or belts of table-lands, and in the disturbances of stratification. The primary division of the surface into areas of land and water are attributed to the assumed smaller conductivity of materials underlying the land, which have been left behind in the general convergence of the surface toward the center. Regarding these as the main and underlying premises of the contractional argument, it is considered unnecessary to state the various subsidiary propositions which have been advanced to explain the determination of this action to particular phenomena, since the main proposition upon which they are based is considered untenable.

There can be no reasonable doubt that the earth-mass consists of a cooled exterior inclosing a hot nucleus, and a necessary corollary to this must be secular cooling, probably accompanied by contraction of the cooling portions. But when we apply the known laws of thermal physics to ascertain the rate of this cooling, and its distribution through the mass, the objectionable character of the contractional hypothesis becomes obvious. As the process under discussion must have continued during an immense duration, we must select some starting point at which the mean temperature of the mass was materially greater than at present. Assuming the degradation of temperature to have been continuous, we have only to assign a duration of sufficient length to arrive at a period when the whole mass, or a considerable portion of it, was fluid. The selection of this starting point is not altogether one of choice, but the one pointed to by the only obvious mode of reasoning, and in itself apparently not inconsistent with present facts. It is quite foreign to the purpose to inquire how the earth originally received its store of heat, since the fact of possession covers all grounds of present inquiry. As was indicated by Sir W. Thomson, the distribution, both of materials and heat, under the condition of fluidity would be an approach toward homogeneity, resulting from movements of convection taking place through the liquid mass. The first stage of evolution resulting from loss of heat would apparently be that of consolidation. The argument of Hopkins is here accepted, that consolidation must begin at the center, as a consequence of the fact that pressure elevates the congealing point; and temperatures being kept nearly uniform, the maximum pressure would determine the primary point of congelation. The solidification of materials at the surface would result in sinking by their increased density until the central solidification had proceeded so near the surface as to leave only an imperfectly liquid mass in which such

movements, becoming more and more retarded, at length ceased, leaving a globe, departing from uniform temperature by differences not greater than the differences in congealing temperatures due to differences in pressure. The result would be a solid globe, with, perhaps, isolated reservoirs of liquid matter, which may have separated in the transition stage from fluid to solid by reason of a higher melting point.

This assumption of the genesis of the earth, though regarded as preferable to all others that have been proposed, is by no means insisted on. It is selected because it gives to the contractional argument the fullest scope and widest range of conditions consistent with known physical laws. There is apparently no supposition which can reasonably allow a higher interior temperature consistently with the formation of a stable surface. To assume a lower temperature for the interior would take away from that argument *pro rata* a portion of the possible diminution of volume upon which it must rely to account for surface corrugation. To assume a higher one would be a violation of physical laws as we now understand them, and virtually constitute an inadmissible appeal to mysteries. Starting, therefore, from a globe possessing the highest degree of temperature which can properly be conceded consistently with a condition in which the evolution of surface features can begin, it remains to inquire how far the cooling has progressed at the present time; what portions have been sensibly affected by it; and to what extent each portion has been affected. Difficult as this problem may seem, it is not beyond the reach of a general solution; and a particular solution will become possible immediately upon the determination of certain data not wholly beyond the reach of experiment.

Sir W. Thomson has very happily called Fourier's solutions of this problem a "mathematical poem;" and the discussion of one of them * by that preëminent philosopher is here summarized. Fourier's problem was that of "finding at any time the rate of variation of temperature from point to point, and the actual temperature at any point, in a solid extending to infinity in all directions, on the supposition that at an initial epoch the temperature has had two different constant values on the two sides of a certain infinite plane."

Let V denote half the difference of the two initial temperatures.
v half their sum.
t the time.
x the distance of any point from the plane.
T the temperature of the point x at the time t.
$\varkappa$ the conductivity of the material in terms of its own thermal capacity.

* Transactions Roy. Soc. Edinburgh, vol. xxiii.

Assuming temperature to be dependent upon the value of x, the rate of variation would be expressed by the first differential coefficient $\frac{dT}{dx}$, whose value, acccording to Fourier, is

$$\frac{dT}{dx} = \frac{V}{\sqrt{\pi\kappa t}} \times \varepsilon^{-\frac{x^2}{4\kappa t}},$$

while the actual temperature at that point would be

$$T = v + \frac{2V}{\sqrt{\pi}} \int_0^{\frac{x}{2\sqrt{\kappa t}}} dz \varepsilon^{-z^2}.$$

These formulæ for the supposed infinite solid are obviously applicable for a limited period of time, and without sensible error, to the cooling earth. Beyond that period it would be necessary, in order to preserve their applicability, to introduce the proper modifications rendered necessary by the spherical form of the earth. It will soon appear, however, that the present, and consequently all past, geological epochs lie far within this limiting period, and hence we need not concern ourselves with it; in other words, the cooling of the earth, comparatively speaking, has made but very little progress up to the present day. To obtain from these expressions determinate values of $\frac{dT}{dx}$ and T, it is first necessary to assign some value to κ, the coefficient of conductivity. To find this Thomson and Forbes instituted a series of experiments upon three different qualities of rock material, by burying thermometers to depths varying from 3 to 25 feet, and observing the effects of the variations of atmospheric temperature upon them. The observations, extending through fourteen years, were carefully reduced for each to the sum of a number of terms, each of which expressed a "simple harmonic," or vibration of temperature. By comparing the amplitudes of the annual vibrations at different depths, the value of the conductivity was determined * for the materials experimented upon. For a mean value of κ, Thomson took 400 as the most probable one; the units being the foot, the degree F°, and the year. This value, substituted in the first equation, gives

$$\frac{dT}{dx} = \frac{V}{35{\cdot}4} \cdot \frac{1}{\sqrt{t}} \cdot \varepsilon^{-\frac{x^2}{1600t}}.$$

It is also necessary to find some value for V, a matter of some difficulty. In the present case this will represent the maximum temperature of the interior at the beginning of the

* That is, the value in terms of its specific heat. The specific heat was determined by Regnault from blocks sent to him for that purpose.

cooling, and its value must be hypothetical. We are concerned, however, only with such values of it as may have undergone change, and we may take it to be the melting point of the more refractory materials which constitute the chief bulk of the nucleus. Presuming these to be anhydrous silicates for at least 500 to 800 miles in depth, and paying due regard to the effect of pressure upon the congealing point, we may accept Sir William's estimate of this temperature, which he takes to be, at a maximum, 7,000° F—a most abundant estimate. This reduces the expression to a relation between three unknown quantities, x, t, and $\frac{dT}{dx}$. If we desire to ascertain the rate of variation of temperature per foot of depth, at the distance x, after the lapse of the time t, (subject to data already given,) we have merely to substitute the numerical values taken for those quantities. Thus the variation of temperature at the depth of 2,000 feet, after the lapse of 100 million years, would be $\frac{dT}{dx} = \frac{V}{354000}$* $= \frac{1}{50{\cdot}6}$ of a degree F per foot of descent. For the same value of t, the rate of increase of temperature at 400,000 feet of depth would have diminished to $\frac{1}{141}$ of a degree per foot; at 800,000 feet, to less than $\frac{1}{282}$ of a degree per foot; and below 150 miles the increase of temperature would not be sensible. For points very near the surface—say ten miles or less—the exponential factor becoming sensibly equal to unity, the equation shows that the increase of temperature would be inversely as the square root of the duration of the cooling. If it be possible, therefore, to determine a true mean rate of increase of temperature per foot of descent at any point near the surface, the time required to elapse from the epoch of the first establishment of the cooling to the present will become known. This mean is placed by some investigators at $\frac{1}{50}$ of a degree F per foot, and by others at $\frac{1}{60}$; the former giving about 100 million, and the latter about 130 million, years.

The accompanying graphical representation† exhibits the law of increase of geothermal temperatures in accordance with Sir W. Thomson's discussion of Fourier's theorem.

Of the general correctness of this theorem there can be no doubt. We may, however, for the moment qualify this assertion by an inquiry as to the nature of one of the quantities entering the expression of it. The coefficient of conductivity κ is regarded as a true constant. Whether it is so in reality is questionable. Experiments upon the conductivity of some materials show that there is probably a variation,‡ which is

* The exponential factor having become so near $e^0 = 1$ that it may be omitted.

† From the paper of Sir W. Thomson referred to.

‡ This has been shown by Principal J. D. Forbes to be the case with iron, the conductivity diminishing with the temperature.

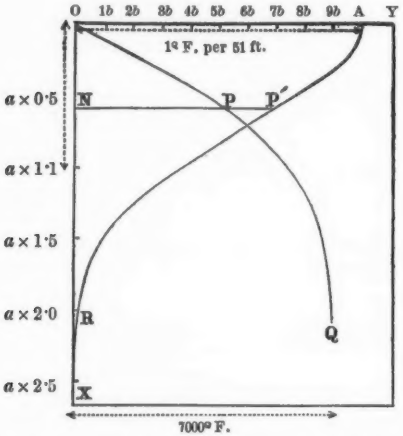
some function of the temperature. But if a function of the temperature, it must also be a function of the time and depth, and hence would alter the general form of the law, and affect all quantitative evaluations derived from it.* The general

Graphical representation of increase of temperature downward in the earth. (*Sir W. Thomson.*)

$ON=x$ $\qquad a=2\sqrt{kt}.$

$NP'=b\varepsilon^{-\frac{x^2}{a^2}}=y'.$ $\qquad \frac{dT}{dx}=\frac{V}{a}\cdot\frac{NP}{b\frac{1}{2}\sqrt{\pi}}.$

$NP=\text{area } ONP'A\div a=\frac{1}{a}\int_0^x y'dx.$



O P Q showing excess of temperature above that of the surface.
A P′R showing rate of augmentation downward.

tendency of the results of experiment seems to be toward the conclusion that conductivity diminishes as the fluid state is approached, and if we are to adopt that conclusion in this case, the quantitative effects would be a smaller totality of dissipation of heat, and a more rapid transition from cold to hot, thus bringing the nucleus nearer the surface. The time required to establish an increase of $\frac{1}{5}\cdot\frac{1}{5}\cdot\frac{1}{5}$° F per foot of descent would also be increased, and probably in no small degree, depending of course upon the amount of variation which the conductivity undergoes with change of temperature. Such a modification

* The identical curve here given might still be used with a slight modification in the axis of abscissas. If instead of equal divisions of that axis, we were to take unequal ones with values determined by the equation $\kappa=f(T)=f'(x)$, the curve would be totally unchanged. To reduce it to a scale of equal parts, stretch out or shrink up, as the case may be, the whole projecting plane in the direction of x until the divisions become equal.

of Fourier's law would still further reduce the basis of the contractional hypothesis by reducing the total dissipation, and the amount of contraction which could be inferred from it.

Again, the value given to κ by Sir W. Thomson has been questioned, and it has been argued that rocks, porous and saturated with water, are much worse conductors of heat than those experimented upon by that philosopher, and there may be good reason for placing this value considerably lower—say at 250 instead of 400. The effect of this modification would be to move surfaceward the positions of the isogeotherms, as determined by the value of the surface rate of increase, and to extend the duration of the cooling.

Another serious quantitative modification will appear possible when we inquire as to the value of $\frac{dT}{dx}$ for places near the surface, i. e., the rate of increase of temperature per foot of descent. This is the yard-stick, by which everything else is measured. Its value is known to vary widely, being $\frac{1}{115}$ in some places, and not more than $\frac{1}{1160}$ in others. Is it safe, or even proper, to take the average of all observed rates as the true and most probable one? It is a most natural inference that the higher ones are attributable to the proximity of exotic masses, are therefore accidental and should be excluded from averages. The extreme slowness with which heat is dissipated from such masses ought to prepare us for the possibility that proximity to exotic igneous masses may vitiate any result. It would seem most proper to select such values as may have been determined in places which show the minimum disturbance throughout their whole geological history. But there is a difficulty even here. The aqueous circulation below the surface, everywhere so abundant, must in some cases affect the normal temperature produced by secular cooling. While a preference is here expressed for the smaller rates, it is yet immaterial, so far as the present argument is concerned, which of the extremes be taken.

1. Given then this world of ours, once of nearly uniform temperature, 7,000° F, now exhibiting an increase of $\frac{1}{160}$ of a degree per foot of descent near the surface. What is the present thermal distribution, and what has been the period of its evolution? The epoch would be about 625 millions of years. At a depth of 300 miles the increase of temperature would be about $\frac{1}{54160}$ of a degree per foot of descent. Thence inward the total amount of cooling since the beginning would be inconsiderable: outward it would show an increasing amount, very gradual at first, but becoming more and more rapid, till it reaches the present mean temperature at the surface.

2. Or take the present surface rate at $\frac{1}{50}$ of a degree per foot, the other conditions being unchanged. The epoch would be about 160 millions of years, and below 140 miles the rate of increase would be inconsiderable.

3. Taking Sir W. Thomson's valuation of κ at 400, instead of 250, and of the surface rate at $\frac{1}{50}$, the epoch becomes about 98 million years, and below 150 miles the rate of increase would be less than $\frac{1}{500}$.

4. Take κ at 250, and $\frac{dT}{dx}$ at $\frac{1}{200}$ at the surface: the epoch would be 2,500 millions of years, and below 600 miles the cooling may be disregarded.

That Fourier's theorem, under the general conditions given, expresses the normal law of cooling, is admitted by all mathematicians who have examined it. The only ground of controversy must be upon the values to be assigned to the constants. But there seem to be no values consistent with probability which can be of help to the contractional hypothesis. The application of the theorem shows that below 200 or 300 miles the cooling has, up to the present time, been extremely little: were it otherwise, the present rate of increase of heat per foot of descent would be lower than the lowest reasonable estimate, unless indeed new evidence can be brought up to show that this ratio is much less than $\frac{1}{100}$, and that the present accepted mean of $\frac{1}{50}$ to $\frac{1}{60}$ is the result of unknown perturbations, tending to exaggerate its value many times. At present, however, the unavoidable deduction from this theorem is that the greatest possible contraction due to secular cooling is insufficient in amount to account for the phenomena attributed to it by the contractional hypothesis.

So far the discussion has taken no account of such inequalities in the process of cooling as have occurred in the form of Plutonic action. Our knowledge of this subject, especially of its history, is so obscure that any treatment we might propose to give it would be purely speculative, and none but the simplest and apparently most necessary inferences from it could be justified. It is certain that such action would accelerate the dissipation of heat, but at the expense of what regions can be known only when we are able to locate the seat of its causation. But in any event it is not apparent that the conclusion just drawn would be sensibly affected in this quarter.

There is, however, a possible source of diminution of volume, other than the contraction directly due to the fall of temperature, which may be alluded to. A change of temperature is, in most cases, followed by changes in chemical relative potentials developing new affinities. In localities where molecular mobility is possible, these changes may give rise to new

compounds of higher average density. But it is not clear how such changes could take place at depths greater than those assigned as the limits of sensible cooling, and such an assumption must appear gratuitous until supported by evidence. The want of such evidence compels us to confine possible changes of density (so far as strict reasoning is concerned) to horizons not lower than two or three hundred miles. Although no estimate can be made of the contraction of this portion, it is probably safe to say that its volume cannot have diminished so much as one-tenth; and if we were to assign thirty miles as the diminution of the earth's mean radius since the first formation of a cooled exterior, we should probably reach the utmost limit consistent with Fourier's theorem. By far the larger portion of this contraction must have taken place before the commencement of the Paleozoic age. By far the larger portion of the residue must have occurred before the beginning of the Tertiary; and yet the whole of this contraction would not be sufficient to account for the disturbances which have occurred since the close of the Cretaceous. In all mountain regions the disturbances of the strata, which are supposed to be due to tangential compression, are so great, that, in order to account for their plication by this hypothesis, we should be compelled to assume a contraction of some circles of latitude, since the commencement of the Permian epoch, amounting to many hundreds of miles. But when we examine the Laurentian rocks wherever found, their excessively disturbed condition must utterly prohibit the belief that it is the result of secular contraction of the interior. Bearing in mind that a shrinkage of one-fifth of linear dimensions implies an increase of 95 per cent in mean density, and that according to this hypothesis such increase is zero at the surface, it puzzles the imagination to conceive what must have been the condition of the earth mass while the Laurentian sediments were accumulating, if we are to assume that their present distortion is due merely to secular contraction.

The determination of plications to particular localities presents difficulties in the way of the contractional hypothesis which have been underrated. It has been assumed that if a contraction of the interior were to occur, the yielding of the outer crust would take place at localities of least resistance. But this could be true only on the assumption that the crust could have a horizontal movement in which the nucleus does not necessarily share. A vertical section through the Appalachian region and westward to the 100th meridian shows a surface highly disturbed for about two hundred and fifty miles, and comparatively undisturbed for more than a thousand. No one would seriously argue that the contraction of the nucleus

had been confined to portions underlying the disturbed regions: yet if the contraction was general, there must have been a large amount of slip of some portion of the undisturbed segment over the nucleus. Such a proposition would be very difficult to defend, even if the premises were granted. It seems as if the friction and adhesion of the crust upon the nucleus had been overlooked. Nor could this be small, even though the crust rested upon liquid lava. The attempts which some eminent geologists have recently made to explain surface corrugation by this method clearly show a neglect on their part to analyze carefully the system of forces which a contraction of the nucleus would generate in the crust. Their discussions have been argumentative and not analytical. The latter method of examination would have shown them certain difficulties irreconcilable with their knowledge of facts. Adopting the argumentative mode, and in conformity with their view regarding the exterior as a shell of insufficient coherence to sustain itself when its support is sensibly diminished, the tendency of corrugation to occur mainly along certain belts, with series of parallel folds, is not explained by assuming that these localities are regions of weakness. For a shrinkage of the nucleus would throw each elementary portion of the crust into a state of strain by the action of forces in all directions within its own tangent plane. A relief by a horizontal yielding in one direction would by no means be a general relief. We may conceive these forces to be resolved into two sets of components respectively parallel to the two coördinate axes. In the case under consideration the diminution of the intensity of one set of components by weakening the supports from which it thrusts would have the effect of increasing the intensity of the other set of components at right angles to the weakened set. No relief could take place unless it be a relief in all directions. The case in question is not that of the cylindric arch, but nearly that of the dome; and if a collapse is to occur, every terrestrial great circle must contract equally and simultaneously; otherwise great deformations of the earth's normal figure would result. The plications of the Paleozoic rocks do not conform, either in Europe or America, to the consequences here affirmed. These disturbances are localized in long and rather narrow belts, and if they truly represent contraction of certain great circles, then such contraction must have been enormous in arcs perpendicular to the axes of plication, and very little in arcs parallel thereto. Still more discordant is the contractional hypothesis with the Tertiary plications. From Cape Horn to the Behrings Sea is a continuous belt, very narrow for most of the distance, but extremely disturbed throughout. If the parallels of latitude perpendicular to this mighty

range have alone contracted so greatly at this axis, how can we reconcile such an assumption with the fact that the earth's present figure is so nearly an ellipsoid of revolution with an eccentricity due to its mean density and angular velocity. It is here that the analogy of the withered apple fails. If it is corrugated irregularly by shrinkage, it fails to preserve its original figure; and conversely, if it preserves its original figure, it must be corrugated uniformly.

A comparison of this hypothesis with details of surface structure would involve an interminable discussion. The diversity of details is so great that only the most prominent and persistent ones could be properly selected; and as it is intended to bring these into relation with other propositions, their discussion will be omitted here.

ART. XII.—*Descriptions of two new Species of Fishes from the Bermuda Islands;* by G. BROWN GOODE.

IN a collection of fishes, including some seventy species, made at the Bermudas in the spring of 1872, I find two forms apparently undescribed, descriptions of which are given below. As the marine life of the Bermuda group is essentially West Indian in its character, these species may be regarded as additions to the icthyological fauna of the West Indies.

1. *Diapterus Lefroyi*, sp. nov.

This species belongs to the genus *Gerres* as defined by Dr. Günther. It is distinguished from all other members of the genus and family by its relatively greatly elongated form. The body is fusiform, compressed, its greatest height, at the thoracic region, being a little less than one-fourth (·23) of the total length and a little more than one-fourth (·27) of the length without caudal (·89): in *Diapterus aprion*, the most elongated of the species hitherto described, the greatest height is but one-third of the length. The height of the body is uniform under the spinous portion of the dorsal, sloping gently and at a nearly uniform angle above and below to the middle of the caudal peduncle. The height of the body behind the dorsal (·10) is less than one-half, that of the least height of the tail (·06) is one-fourth of the greatest height of the body.

The scales are large, measuring ·03 and ·04 in height and ·02 and ·03 in length: they form about forty-five oblique transverse rows between the head and the caudal, four and one-half longitudinal rows between the back and the lateral line and ten between the lateral line and the belly.

The length of the head (·22) equals the greatest height of the body and is double the greatest width of the head (·11): the height at the pupil (·14) is double the width of the interorbital space (·07). The length of the snout (·06) equals the length of the operculum (·06); when the mouth is protruded the length of the snout is doubled (·12) and when retracted the posterior extremity of the intermaxillary process extends to the vertical through the center of the pupil. The nasals are very prominent and the nostrils are nearer to the orbit than to the extremity of the jaw.

The orbit is circular, its diameter (·08) one-third the length of the head. The origin of the dorsal is slightly behind that of the ventrals, its distance from the snout (·31) twice the length of its base (·16). The dorsal spines are graduated nearly in the proportion (I=·02; II=·12; III=·11; IV=·10; V=·09; VI=·085; VII=·0725; VIII=·05; IX=04). The notch between the spinous and soft portions is very deep and the connecting membrane barely perceptible. In the soft dorsal the fifth ray is the longest (·09) and equals the fifth spine, the succeeding rays diminishing regularly to the last, which equals the ultimate spine (·04); the length of its base (·20) is greater than that of the spinous dorsal. The anal begins behind the center of the body (·56); the first spine is very short (·01), one-fifth the length (·05) of the second, which is slender; the first ray is the longest (·08), the succeeding rays regularly diminishing in length to the last (·03). The lobes of the caudal are equal, the outer rays in length (·21) five times the inner ones (·04). The extremity of the pectoral reaches the vertical from the last dorsal spine: its distance from the snout at the axilla (·25) is nearly equal to the height of the body. The ventral spine resembles the fifth dorsal spine in shape and size; the length of the longest ray (·11) slightly exceeds one-third of the distance from the snout to the ventral axilla (·30); the axillary appendage consists of four lanceolate scales, the first and longest as long as the last ventral ray.

Color: silvery, with a bluish tint above; axils of the pectorals and extremity of snout brownish.

Radial formula, D. IX, 10. A. II, 8. P. 12. V. I, 5. C. 3, 9, 9, 3.

The unit of measurement used above is one-hundredth of the total length, which in an average specimen is 7·29 inches (M. O. ·185). The species is common in the protected inlets about the islands in company with the "shad" (*Diapterus gula*), from which it is distinguished by the name "long-boned shad:" they are in demand for bait and are easily seized in large quantities. I take pleasure in dedicating the species to his Excellency, Maj.-Gen. J. H. Lefroy, F.R.S., Governor of the Ber-

mudas, who while doing so much for the social and political welfare of the islands, is taking an active part in adding to our knowledge of their natural history.

2. *Engraulis chœrostomus*, sp. nov.

This species closely resembles *Engraulis surinamensis* (Blkr.) Gthr. differing from it, however, in several respects.

The height of the body (·16) is a little more than two-thirds of the length of the head and is contained six times in the total length and a little more than four times in the length to end of middle caudal rays (·90): the height at the ventrals is less (·13). The scales are large, in thirty-eight oblique rows between the head and the caudal.

The length of the head (·22) is less than one-fourth of the total and is double its height at the pupil (·11): its greatest width (·08) is about one-third of its length. The orbit is nearly circular and its diameter (·05) equals the length of the snout (·05) and the width of the interorbital area (·05). The snout projects far beyond the lower jaw, whose extremity just passes the vertical from the anterior margin of the orbit. The maxillary is dilated above the mandibular joint, rather tapering behind, and extends to the gill opening. The gill-rakers are fine, setiform, not longer than the eye (·05), about 25 on the lower branch of the outer branchial arch.

The origin of the dorsal fin is in front of the middle of the body (·45 from snout), and directly above the extremities of the ventrals: the length of the first ray (·06) is half that of the second (·12), which nearly equals the length of the base (.11).

The origin of the anal is at the middle of the body (·51 from snout) and below the posterior dorsal rays: its greatest height (·11) nearly equals that of the dorsal.

The length of the middle caudal rays (·08) is two-fifths of the outer rays (·20). The length of the pectorals (·11) equals the length of base of dorsal (·11), the extremities reaching to the origin of the ventrals. Length of ventrals (·09): distance from snout (·35).

Color: back and sides brownish, belly white; a broad, clearly defined lateral band of silver as wide as the diameter of the orbit (·05).

Radial formula D. 13–14. A. 23–24. Length 2·68 inches (M. O ·068).

Common in schools in Hamilton Harbor, where it is taken for bait in cast nets. Its enormous mouth has given it the name of "hog-mouth fry."

The types of these descriptions are preserved in the U. S. National Museum in Washington and the University Museum in Middletown, Conn.

ART. XIII.—*On an optical method of studying the Vibrations of Solid Bodies;* by OGDEN N. ROOD, Professor of Physics in Columbia College.

IN the year 1855 Lissajous described a beautiful mode of bringing two tuning-forks into exact unison, which since that time has been of great use for the production of exact copies of standard forks executing a known number of vibrations in a second. This method is now so familiar that farther allusion to it would be superfluous, and I pass on to the description of an analagous proceeding, which if it falls a little short of that of Lissajous in point of exactitude, is, on the other hand, more easy of execution and more generally applicable to the study of the vibrations of solid bodies of very different forms. The nature of the method referred to will best be illustrated by a few examples.

Tuning-forks.—Let us suppose that it is required to ascertain whether two forks are in unison, or to determine the difference in the number of vibrations executed by them in a second. For this purpose a short piece of fine steel wire is attached to each of the forks and they are supported in positions so that their vibrations shall be at right angles to each other, as indicated in fig. 1. The wires may have a diameter of one or two-tenths of a millimeter, or even less, and are to be attached with the least possible amount of soft wax or varnish. They may be brought quite near to each other, or may, if necessary, be several inches apart. If the forks are now set into vibration and the intersection of the wires viewed against a bright background with a small telescope, it will be seen that an optical figure is developed, which is partly due to the same well known conditions that give rise to the figures of Lissajous, and partly to the circumstance that the wires move with less velocity when near their maximum deviation from the line of rest. Hence, if the difference in phase is 0, an appearance like fig. 2 is produced, which changes into fig. 3 when the difference in phase has increased to one-half a complete vibration. Fainter indications of the same figures are shown in all cases, except when the difference in phase is one-fourth, three-fourths, &c., of a vibration, or nearly so. This figure, then, is characteristic

of forks in unison, and by proper arrangement of the light and telescope can be made tolerably sharp and distinct. If the forks are actually in unison, the above figure is pretty certain to be produced after one or two trials, and the fact of its constancy will then, on the other hand, be the evidence of perfect unison. If the forks are not exactly in unison, fig. 1 will, as stated, after some time change into fig. 2, and the number of seconds necessary for this change will measure the interval required by one of the forks in gaining or losing half of a complete vibration.

When the two wires are only a few millimeters apart, it is evident that the telescope will furnish distinct images of them both without any especial contrivance, but as a general thing in the application of this method, the intervening distance between the vibrating bodies will be greater, giving rise to an obstacle in making the focal adjustment. This is readily met by limiting the aperture of the object-glass. The focal length of the object-glass of the telescope used by me was 120 millimeters for parallel rays, and when the aperture was reduced to two millimeters, sufficiently distinct vision of both the wires could be obtained, even when their distance apart was several centimeters, and the telescope yet so near to them as to admit of the performance of all the manipulations by the observer without rising. With this limited aperture, the light from a white cloud answered quite well.

If the forks differ by an interval of an octave, an almost equally distinct and well marked figure will be produced, such as is seen in figs. 4 and 5, which represent the characteristic appearances in this case. This figure is quite as useful for purposes of investigation as that of unison. Somewhat less distinct and more complicated figures are given by the quint, the duodecime and the double octave. It is a little more difficult to distinguish them from each other by a glance, and they are less sharply defined than those of unison or of the octave, which should always have the precedence for experimental determinations; still the other figures admit of being employed when necessary. The relation of all these to the corresponding figures of Lissajous is evident on inspection.

Tuning-forks and vibrating cords.—From the foregoing it evidently is easy with this method to bring a vibrating string into unison with a given tuning-fork, or to adjust it so that the interval shall be a quint, octave, twelfth or double octave, above

or below. It is also easy to ascertain the number of vibrations made by a string in a given case, by the aid of a bridge and a properly selected fork making a known number of vibrations, the string being shortened till it furnishes one of the above mentioned figures, and executes hence a known number of vibrations, after which the number of vibrations made by its whole length can readily be calculated by a well known law.

Vibrating cords.—To bring two cords into unison, or to produce one of the above mentioned intervals, it is not at all necessary that they should actually vibrate at right angles to each other: in my experiments I simply placed between the strings on the monochord a cork cut at an angle of 45°, and supporting at this angle a small piece of looking-glass of good quality. The reflected and vertical image of the farther string was then seen in the telescope crossed by the horizontal image of the nearer string, and the mirror being turned so as to reflect at the same time light from the sky, all the conditions were fulfilled. It is evident that this arrangement furnishes an excellent experimental mode of studying the laws of vibrating strings, of comparing them with theory, and examining into the deviations caused by stiffness and unequal caliber.

In all the experiments with strings I employed the differential *sonomètre* of Marloye as constructed by König, so well known for his excellent workmanship, merely adding a clamp to one of the bridges for the purpose of holding down the cord. But this instrument, although admirable when the ear is used as a test, was found not to possess a delicacy sufficiently great to enable the experimenter to take full advantage of the sensitiveness of the method here described. The arrangement for varying the tension was hardly sufficiently fine, and a screw movement attached to the bridge would have been a great advantage. It was also found difficult to maintain a given tension with perfect exactitude for any length of time. Probably some modification of the older arrangements of Weber or Fischer would be found to answer better.

Vibrations of rods, bars and plates.—It is evident that rods or bars supported at one extremity or at two nodes, and provided with fine terminal wires, can by this method be brought into unison, or have one of the above mentioned intervals established between them. A preferable mode, however, is to study them in connection with the monochord and a tuning-fork. The entire string of the monochord is first brought into unison with a tuning-fork, or some definite interval established; the cord and rod or bar are then combined at right angles, and the bridge moved till unison is again effected, when it is possible to calculate the number of vibrations actually executed by

the bar or plate. As an example, I give the result of two rough experiments, which would have been rendered more accurate by the aid of an assistant.

The cord, one meter in length, was brought into unison with an Ut_1 fork, and hence executed 64 double vibrations per second. It was afterward combined with a plate of glass 330 millimeters long and supported at the two nodes. Five determinations with the bridge were made, and after bringing the cord a second time into unison with the fork, repeated.

847·	843·
846·9	846·5
847·7	846·9
847·1	845·7
847·7	843·
847·28	845·02

The result then in the first case was 75·535, in the second 75·738 vibrations per second.

An experiment with another piece of glass cut from the same plate and of nearly the same length gave, with two determinations, 77·811 and 77·717 vibrations per second.

Vibrations of bells.—If the fine wire is attached to one side of a bell, the number of vibrations executed by the bell can readily be obtained with the monochord in the manner already indicated. A bell-glass was set in vibration by a bow, and combined with a string one meter in length, which was an octave lower than a Sol_2 fork, and hence made 96 double vibrations per second. The bridge was adjusted till the string was an octave lower than the bell-glass when sounding its fundamental note. The results are given below:

802·	803·
802·7	803·5
804·5	804·
804·2	801·7
803·5	802·2
803·38	802·88

The number of vibrations obtained in first case then was 238·99, in the second 239·14, with a difference of ·15 of a single vibration.

In experiments of this kind it is plain that the accuracy attainable depends to a great extent on the time during which the vibration of the two bodies can be maintained; still it is not admissible to maintain the cord in vibration by the help of the *bow*, as the slightest variation in the pressure causes the

figure at once to change. On the other hand, the bow is useful in bringing a short string into unison with a fork, &c., merely for the purpose of assuring the experimenter that the figure actually to be used, and furnished by a *greater length* of the string, is really that of the lower octave, twelfth or double octave. In the experiments with the monochord the string was simply drawn aside with the feather-end of a quill, and then left to itself. If this and the vibration of the plate, &c., be effected by the aid of an assistant, the time for observation at the telescope is increased and more accurate results than those above given can be obtained. The changes in the plane of vibration of the string are hardly a source of embarrassment when the unison or octave figures are employed, but become so with the others which are more complicated, in the case where it is desired to count the changes occurring in the figure during a certain number of seconds.

Vibrating membranes I find can readily be studied in this way by attaching to them a small piece of fine wire bent with two right angles, and using them in connection with the monochord or a tuning-fork.

Finally, I may add that the more important of these figures may easily be rendered visible to a large audience. Wires about a millimeter thick are attached to two tuning-forks placed in front of a magic lantern; an image is formed on the screen with the aid of a lens of about eighty millimeters focal length; the figures are then well shown, along with certain of their details not particularly mentioned in this article.

New York, May 21st, 1874.

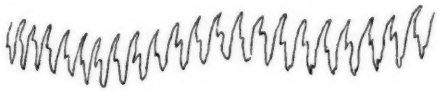
Art. XIV.—*The Phonautograph;* by Chas. A. Morey.

Almost every collection of acoustical apparatus includes one of Leon Scott's phonautographs, but I think I am right in saying that very little use is ever made of them, other than for their explanation. The curves being drawn upon blackened paper cannot be projected, and in most cases they are of so small an amplitude that they require very close inspection for their analysis. As was found at the time of its invention, the great difficulty lies in the fact that the principal motion of the style is a longitudinal one instead of a lateral one. All these objections are obviated, and the instrument rendered extremely useful, by the following simple device, which may be readily applied to any one of them. Instead of attaching the style directly to the membrane, it is attached to the end of the long

lever. This may be of any length (the one used was about twelve inches) and is made very slender and light, either of deal or of a stiff straw. This is attached at the end to the brass ring which holds the membrane by a simple hinge of goldbeater's skin, and also to the membrane itself by a short, flexible bristle, or piece of broom-corn. This is inserted into the lever, and is fixed with a drop of glue; the other end is fixed to the membrane as the style usually is, either by a drop of glue, or sealing wax, the former being preferable. Now it is evident that the former longitudinal motion of the style will impart a lateral motion to the lever, and thus to the style at the free end of it; and also that this motion will be greatly magnified. The curve can now be drawn upon a plate of smoked glass, as in the well known experiments with tuning forks, the plate being drawn under the style in the direction of the length of the lever. The whole attachment can readily be made so light as to encumber the membrane but very little.

The following curves will serve to show the advantage gained over the usual attachment, as well as to suggest the

1.

2. 

3.

numerous uses to which the instrument can be put. The first one is the toungue trill, or the German *r* prolonged; the second is the result of a note upon the trombone; and the third is the *oo* in mood. The fact that a difference in the intensity of the sound shows itself so very plainly in the curves (2), seems to suggest something in the way of quantitative work in that direction, but it is yet a question as to the delicacy of the instrument.

In each of the cases above, the sound was made in front of the parabolic condenser, but at some little distance from it—two or three feet.

Mass. Inst. Technology, Boston, May 24, 1874.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *The so-called Antimony blue.*—A year or more ago, a new coloring matter was introduced into commerce under the above name, prepared by precipitating a solution of antimony in aqua regia by potassium ferrocyanide. Inasmuch as this re-agent is commonly used by chemists to detect the presence of iron in antimony, it is obvious that the blue compound is not ordinarily thrown down by it in solutions of this metal. Krauss has therefore investigated the conditions of its production. He finds that on boiling a solution of tartar-emetic in concentrated hydrochloric acid, to which some ferrocyanide of potassium is added, the same blue substance is produced. On analysis no antimony could be detected in it. Believing therefore, that the antimony was in no wise essential, Krauss boiled the ferrocyanide in concentrated hydrochloric acid alone, and obtained the blue compound. In further proof of this assumption, he found that the antimony could be satisfactorily replaced by mercury. The use of antimony accelerates the formation of the precipitate. The blue substance is soluble in hydrochloric acid and is decomposed by it on boiling, producing ferric chloride. Water gradually restores the color. It is not soluble in water, is at once decomposed by caustic alkalies, and in powder has the coppery reflection of prussian blue. It is therefore only another of the blue compounds of iron and cyanogen, and the name "antimony blue," given to it is a misnomer.—*Moniteur Scientifique*, III, iii, 1095, Dec., 1873. G. F. B.

2. *On the Alloys of Hydrogenium with Palladium, Potassium and Sodium.*—Troost and Hautefeuille have sought to determine whether palladium-hydrogenium is a mechanical or a chemical combination, by studying the tension of the hydrogen evolved from it at various temperatures. Chemical compounds having a gaseous constituent, suffer, as is well known, a partial decomposition under the influence of heat, the tension of the gas evolved being invariable for each temperature, and having no relation to the amount of the undecomposed product; while on the other hand, solutions of gases in solids, as hydrogen in platinum black, for example, being mechanical mixtures, evolve, when heated, a gas whose tension, at the same temperature, is variable, being dependent on the degree of saturation of the absorbing substance. Palladium, charged with hydrogen electrolytically,* was placed in a tube of glass, connected at one end with a manometer, and at the other with a Sprengel pump. Operating at the temperature of 100° C., with palladium charged in different degrees, the results show: 1st, that when the volume of the absorbed hydrogen is

* Since palladium thus saturated loses hydrogen at the ordinary temperature, and since on exposure to the air, it heats, from the oxidation of this gas, the authors adopted the plan of placing it at once in water free from air, and heating this to boiling, the gas evolved being measured. It was then treated as above.

above 600 times that of the palladium, the tension decreases very rapidly with the decrease in the amount of hydrogen, a fact characteristic of a simple solution. 2d, that the tension becomes constant, the proof of chemical union, so soon as the volume of hydrogen reaches 600 times that of the palladium; a volume which corresponds to one-half an atom of hydrogen to an atom of palladium. From this limit, palladium-hydrogenium behaves like a definite compound, capable of true dissociation, the gaseous tension henceforth depending alone on the temperature. The authors have prepared a table of the tensions of dissociation at different temperatures, from which it appears that the compound Pd_2H evolves no gas at ordinary temperatures and that at 130°–140°, its tension is equal to the atmospheric pressure; hence the compound cannot be formed at temperatures superior to this, except under pressure. Moreover, the authors have proved that Pd_2H is capable of dissolving hydrogen, the quantity depending on its physical state.

In a second paper, Troost and Hautefeuille give the results of their investigations upon the definite compounds which potassium and sodium form with hydrogen. The potassium was placed in an iron boat, in a glass tube connecting by a T piece with a manometer on one side and a three-way cock on the other, by means of which the tube could be exhausted, or filled with pure and dry hydrogen. This tube could be heated to any desired temperature and kept there for any length of time. The authors found that no absorption took place when the potassium was melted, nor below 200°; at 290°, 2·5 grams required 250 hours for saturation; while at 350° to 400°, the absorption goes on much more rapidly. Potassium-hydrogenium thus prepared, is a brittle substance, having the crystalline grain and the luster of silver amalgam, and appearing like a true alloy. It may be fused in hydrogen or in a vacuum without change. In the air it inflames spontaneously. At 200° in a vacuum it begins to dissociate, the tensions being measurable between 330° and 430°, and increasing very rapidly with the temperature above 370°, becoming 760 mm. at 411°. Hence it cannot be formed at this temperature, except under pressure. The best temperature for its formation is 300°. It dissolves hydrogen, the quantity varying with the temperature and pressure; at 300° and 760 mm. it dissolves 40 volumes of this gas. On expelling the excess of gas, a point determined by the agreement of the tension of the hydrogen with its dissociation-tension for that temperature, the compound gave on analysis 126 volumes of hydrogen to one of potassium, the formula K_2H requiring 124·6. Sodium absorbs hydrogen actively between 300° and 421°, the tension at this latter point being 760 mm. At ordinary temperatures sodium-hydrogenium is soft, but becomes brittle and crystalline just before fusion, so that it may be pulverized. It has the luster of silver, is a little more fusible than sodium, loses no hydrogen on fusion and is only slowly alterable in the air. Its density taken in naphtha is 0·959, that of sodium being 0·970. Its dissociation

follows the same laws as the potassium compound, the tensions being a little less for the same temperatures. It absorbs 3 or 4 volumes of hydrogen at 400° and 760 mm. Analysis gave 237 volumes of hydrogen; Na_2H requires 238. Lithium, heated to 500° in hydrogen under 760 mm., absorbs 17 times its volume of this gas. Thallium, under similar conditions, only three times.

In a third paper, Troost and Hautefeuille give the results of their determination of the density of this solidified hydrogen. Regarding, with Graham, these compounds as alloys of metallic hydrogen, and assuming that they, like the alloys examined by Matthiessen, are formed without condensation, it is easy to calculate the density of the hydrogen as contained in them. Graham's number 0·733, for the density of hydrogenium, is too high, since, as we have seen, he operated on a substance which disengaged hydrogen at ordinary temperatures and was not a definite compound. The authors, taking their compounds Pd_2H and Na_2H, determined accurately their densities. The density of the former they found to be 11·06, that of the palladium being 12. From these data, the density of hydrogenium, as calculated, is 0·62. The density of the Na_2H is 0·959, that of the sodium being 0·970. This gives 0·630 as the density of hydrogenium, a remarkably close accordance. The mean is 0·625, a density but a little superior to that of lithium. The atomic volume of hydrogenium, as deduced from the above data, is the smallest known, being 1·6.—*C. R.*, lxxviii, 686, 807, 968, March, April, 1874.

G. F. B.

3. *On a Lactic acid of the Allyl series.*—In the hope of reducing trichlor-lactic acid—obtained by heating chloral-cyan-hydrate with hydrochloric acid—to monochlor-lactic acid, and of effecting in this way the synthesis of glyceric acid, PINNER treated it first with zinc-dust and water, and subsequently with zinc and dilute hydrochloric acid. But the difficulty of separating the products from the large quantity of zinc chloride formed, rendered this method unfruitful. The acid was then converted into its ethyl-ether and this, treated with zinc and hydrochloric acid, acquired soon a peculiar odor and became converted into a volatile oil, which upon fractioning, boiled constantly at 145°–146°. Analysis showed that it was not monochlor-lactic acid, but that it was ethyl monochloracrylate; not only the two chlorine atoms being replaced by hydrogen but a molecule of water abstracted in addition, thus:

$$CH_2Cl.CHOH.COOC_2H_5 - H_2O = CHCl{=}CH{-}COOC_2H_5.$$

Ethyl monochloracrylate is a mobile colorless liquid, boiling at 146°, having the odor of the allyl series, and irritating the mucous surfaces. Boiled with barium hydrate, it dissolves gradually, exchanging its chlorine for hydroxyl, and yielding the well-crystallized barium salt of a new lactic acid which Pinner calls acryl-lactic acid, and which has the formula $C_3H_4O_3$ (or CHOH.CH.COOH); thus differing from lactic acid by H_2 less. The free acid was prepared from the barium salt, but for want of material was

not fully examined. Ethyl chloracrylate, treated with alcoholic ammonia, gives rise to an alanin, crystallized in fine needles. It has the formula $CH(NH_2)=CH-COOH$, and differs from ordinary alanin not only by having H_2 less, but also by being nearer to ethylene-lactic acid in constitution than to ethylidene-lactic acid, from which alanin itself is derived.—*Ber. Berl. Chem. Ges.*, vii, 250, March, 1874. G. F. B.

4. *Occurrence of Leucin in the fresh juice of the Vetch.*—Both leucin and tyrosin stand in very intimate relations with the albuminates. Their frequent occurrence in the body, their appearance in the urine in certain diseases, their rapid production in the peptonizing of the albuminates, all go to show the importance of these relations. Now, since asparaginic acid occurs among the decomposition products of leucin, and since asparagin appears during the sprouting of the papilionaceæ and disappears later while protein bodies are forming, it occurred to Gorup Besanez to examine, for leucin, the juice of the common vetch, grown in rich earth and in the dark. This juice, freed from albuminates by boiling, was dialyzed, the diffusate being evaporated till the asparagin crystallized out. The mother liquor, on further concentration, deposited a granular substance, which formed crusts on the surface of the liquid, and which had all the appearance of leucin. Repeated recrystallization from boiling alcohol gave it pure, in the form of small spheres of sharp outline and radial in structure. It reactions proved it to be leucin. The author believes with Dragendorff, that the substance obtained by Reinsch from the juice of *Chenopodium album*, and called chenopodin, is really leucin.

In a second paper, Gorup Besanez shows that this occurrence of leucin in the juice of the vetch is constant. The shoots were grown in wet sand and in a dim light, and were examined after two weeks growth, when they were 12–15 cm. long—after three weeks growth, length 20–25 cm.—and after four weeks growth, length about 25 cm. The leucin appeared to exist in inverse ratio to the asparagin. In these experiments the juice, after separation of the protein bodies by coagulation, was mixed with 90 per cent alcohol in excess, thus precipitating, beside other matters, almost all the asparagin. The filtrate on concentration deposited first asparagin, then the leucin; a sugar-like substance, reducing the copper test, remaining in the mother liquor. Since leucin cannot be detected in the ripe seeds of the vetch, and legumin, which these contain, does not exist in the etiolated sprouts, the author believes that the leucin is an intermediate stage in the production of the albuminate. Althea roots and the roots of *Scorzonera hispan.*, were examined for leucin, but without result.—*Ber. Berl. Chem. Ges.*, vii, 146, 569, Feb., April, 1874. G. F. B.

5. *On Protamine, a new Base from the Spermatozoids of the Rhine salmon.*—Miescher has examined the chemical character of the spermatozoids of the Rhine salmon, which at the time of maturity, in November, may be obtained pure in considerable

quantity either from the milt itself or from the glands secreting it. The composition of these spermatozoids, though peculiar, is quantitatively quite constant, containing of lecithin 7·5 per cent, cholesterin 2·2 per cent, fat 3·5 per cent, albuminates 10·3 per cent, nuclein—the principal constituent, containing 9·6 per cent of phosphorus—48·7 per cent. This latter substance, now for the first time obtained pure, is not free in the sperm; it exists there as an insoluble compound of an organic base, protamine. To prepare this, the spermatozoids are extracted with hot alcohol to remove fat, lecithin, etc. The residue may then be treated with hydrochloric acid and precipitated with platinic chloride; or with nitric acid and thrown down by mercuric nitrate. On decomposing the precipitate with hydrogen sulphide, the hydrochlorate or nitrate of the base is obtained. Both these salts crystallize with difficulty on slow evaporation in prisms probably rhombic. They are easily soluble in water, difficulty so in alcohol, insoluble in ether. They have a peculiar taste, which is astringent, faintly sweet and at the same time bitter. Evaporated with nitric acid a yellow spot is left, which on adding sodium hydrate, becomes a beautiful red, passing into violet on warming. The free base reacts alkaline. Analysis fixes its formula as $C_9H_{20}N_5O_2(OH)$. It constitutes 26·8 per cent of the dry spermatozoids. From the testicles of a single large salmon in October, 20–30 grams of protamine may be obtained.—*Ber. Berl. Chem. Ges.*, vii, 376, April, 1874.

G. F. B.

6. *On the Aqueous lines in the Solar Spectrum at high altitudes.*—Croce-Spinelli and Sivel, during their balloon-ascent of March 22d, were furnished by Janssen with a small spectroscope for the purpose of observing the solar spectrum at high altitudes. They ascended from Villette in a balloon of 2800 cubic meters capacity, at $11^h\ 34^m$ in the forenoon, and reached their highest point at $1^h\ 30^m$, the barometer standing at 30 cm., indicating a height of 7300 meters. The temperature, which at starting was 13°, fell to −22°. The descent was safely accomplished at $2^h\ 12^m$. The spectroscopic observations were to be directed specially to the two dark bands on either side of the D lines, produced, as is well known, by the vapor of water. Janssen attributed them to terrestrial absorption, and hence maintained that they ought to disappear at a high elevation. Secchi, on the contrary, believing that the aqueous vapor producing the absorption existed on the sun, argued that they must persist at any altitude. The balloon observations fully confirmed Janssen's view. At 5500 meters, the band on the right of the D line disappeared, and at 7000 meters, that on the left become invisible. The lines E and F were more decided than at the sea level. The red of the spectrum became darker, so that B and C were perceived with difficulty. Atmospheric glare was so much reduced that at 6000 meters, at 180° from the sun, only the yellow of the spectrum could be seen and that without lines. The observations given were made at an angle 5° to 7° from the sun.—*C. R.*, lxxviii, 946, April, 1874.

G. F. B.

7. *Apparent Adhesion.*—M. STEFAN designates by this name the phenomenon that when two plates are laid one upon the other they cannot be again separated without the expenditure of a force. This phenomenon has been hitherto conceived as conditioned by adhesion—that is, by the molecular forces between the particles of the two plates; and experiments have been made for the purpose of determining statically its amount.

In this phenomenon, however, we have not to do with a static, but with a dynamic problem. The experiments made by the author showed that the separation of the two plates can be effected by any force whatever; only the time in which the distance of the plates is changed a measurable quantity by the action of such a force is the greater the smaller the force.

Simultaneously with the commencement of the action of a separating force the distance of the plates commences also to increase; yet the motion is very slow, and grows ever quicker with increasing distance. The apparent adhesion is much greater when the plates are under water or another liquid instead of in air. The distance of two plates of 155 mm. diameter, immersed in water, amounting at first to 0·1 mm., increases, in consequence of the continuous pull of 1 gram, 0·01 mm. in 1½ minutes, 0·1 mm. in 7 minutes. From this it is intelligible how, limiting the observation to a short time, one may be misled to the assumption of a static equilibrium. The author measured, in his experiments, the times which elapsed while a given initial distance measured by a wire placed between the plates increased by a certain quantity. Between these times and the other quantities, which varied with the experiments, the following relations were found. The times are inversely proportional to the separating force; they are nearly, but not exactly, inversely proportional to the squares of the initial distance; for plates of different sizes, they are to one another as the fourth powers of the radii of the plates; for different liquids, as the times in which, under equal pressure, equal volumes of the liquid flow through a capillary tube.

From this it evidently results that with this phenomenon the question is a problem of hydrodynamics; and it is never easy, at least in general, to describe it. When the separating force begins to act, the distance between the plates receives an infinitely small increment; thereby the space limited by the plates is augmented, and the fluid undergoes a dilatation, in consequence of which the hydrostatic pressure becomes less. The excess of pressure of the exterior fluid acts in opposition to the separating force. Nevertheless, equilibrium does not ensue, because the diminution of the hydrostatic pressure between the plates has for its result a flowing in of the exterior fluid and thereby, again, a diminution of the difference of the pressures. The distance of the plates can be again increased by the separating force, and the same process repeats itself in a continuous manner.

The author gives also an approximate theoretical solution of the problem, starting from the following consideration. The *vis viva*

acquired by the plates through the separating force is, on account of the great slowness of the motion, vanishingly small in comparison with the work of that force. This work must consequently have its equivalent in another work; it has it in that which is necessary for maintaining the flow of the liquid from the outside into the space included between the plates.

The equation deduced from this assumption gives again all the different laws to which the experiments have conducted. It permits us to derive from the experiments the coefficients of internal friction for the experimental fluids. If the centimeter be chosen for the unit of length, the mass of one gram as the unit of mass, and the second as time-unit, it follows that for water of the temperature of 19° C. this coefficient = ·0108, for air = ·00183, which values almost exactly coincide with those deduced from the experiments of Poiseuille, Maxwell and O. E. Meyer.—*Royal Acad. of Vienna*, April, 1874; *Phil. Mag.*, xlvii, 465. E. C. P.

8. *Effect of Magnetism on an Electric discharge in rarefied gases.*—MM. De La Rive and Sarasin have in a former memoir (Archiv. des Sci., xli, 5) studied the action of a magnet on a discharge perpendicular to its axis. Since then, they have modified the experiment as follows. The two bobbins of an electro-magnet were placed end to end, and the Geissler tube inserted so as to be acted on by only one of the magnetic poles. Two cylindrical tubes were used, one containing hydrogen, the other nitrogen, at a pressure of 1 mm. or less. The induced current was produced by a large Ruhmkorff coil and a very small portion of the current directed through a galvanometer far enough from the magnet to be uninfluenced by it. Ordinarily, the discharge exhibits around the negative electrode a beautiful blue aureola extending to the sides of the tube, beyond a long dark interval, and thence to the positive electrode streaks wide apart. This appearance is completely changed as soon as the magnet is excited. When the negative electrode is acted on by the magnet, the negative aureola, which previously had a diameter equal to that of the tube, or 32 mm., and length 35 mm., is transformed into a cylinder of only 8 or 9 mm. in diameter, very luminous, extending to the positive electrode, presenting an appearance similar to the narrow positive jet observed at about 8 or 10 mm. pressure.

The same effect was obtained with a large bell, in which a central negative electrode was encircled by a positive ring. The large spherical aureola developed at low pressures, being replaced by a narrow blue jet of vivid splendor, having sometimes the appearance of a brilliant blue flame escaping from the positive electrode.

The effect of the magnet on the resistance of the gas was also very marked. In the case of hydrogen the derived current through the galvanometer was increased from 20° to 40°, when the electro-magnet was excited by 25 Bunsen cells. The corresponding change in the case of nitrogen was from 20° to 30°. In another case, with 40 cells, the change was from 12° to 55° for hydrogen, and 10° to 35°

for nitrogen. When it is the positive electrode that is acted on there is scarcely any appreciable effect. But the kind of magnetism makes no difference.

When the circuit contains several consecutive Geissler tubes, all placed in the same way, each having its negative electrode next the magnet, the effect upon the strength of the current is still greater. But if, in addition to the tube or tubes placed under the action of the magnet, there is one in the circuit out of this action, no effect upon the intensity of its current is produced by the magnet, although the modification which the appearance of the discharge undergoes in the other tubes placed over the magnetic tube, remains the same. It seems, then, that it is a special and peculiarly intense resistance, having its seat at the issue from the negative electrode, which is thus overcome by the intervention of the magnet.

A final series of experiments support this view, and show that the dimensions of the negative electrode, which notably influence the dimensions of the aureola and the resistance to the passage of the electricity, influence also the augmentation of intensity produced by the magnet in the case of an axial discharge. Working with the large bell, a very great loss or almost no augmentation of intensity was obtained according as a platinum point of wire, a small ball, or a ball 4 cm. in diameter was employed as a negative electrode.—*Bib. Univ.*, l, 41; *Phil. Mag.*, xlvii, 462. E. C. P.

9. *Frictional Electricity.*—M. L. Joulin has investigated the laws of frictional electricity with a machine suggested by the electrical phenomena often observed in belts used for transmitting power. A very great quantity of electricity is thus produced, capable of giving long brushes and sparks, of deviating a galvanometer needle, of decomposing water, and in Geissler tubes of showing the stratification of the light. A new method of measuring electric tension has been employed, dependent on the greatest distance at which a brush is perceptible on a given sphere when brought near the electrified body. The experiment was varied by using pulleys of various metals or non-conductors and by changing the velocity tension and temperature. In arranging the results there seem to be three causes influencing the production of electricity. The velocity of separation of the parts of the belt and pulley, the complex mechanical action of bending the belt, and the temperature of the two materials.—*Ann. de Chim. et de Phys.*, ii, p. 5. E. C. P.

10. *Note to Prof. Cooke's monograph on the Vermiculites;* by the Author.—In a note to my monograph on the Vermiculites (Proceedings of the American Academy, vol. ix, and this Journal, vol. vii, page 427,) I described incidentally, certain phenomena of circular polarization obtained by superposing plates of muscovite mica, which have a wide optical angle. Quite recently my attention has for the first time been called to the fact that these phenomena had been previously observed by E. Reusch (Monatsberichte d. Akad. Berlin, July, 1869), and I take this early opportunity to acknowledge his priority.

The observations of Reusch do not appear to have received that notice which their importance deserved; and, by exhibiting the same results from a different point of view, I hope I have been able to bring them into greater prominence, and to show their important mineralogical bearing.

Cambridge, July 1, 1874.

11. *Will's Tables for Qualitative Chemical Analysis.* (2d American, from the 9th German Edition.) Edited by Professor C. F. HIMES, Ph.D. 8vo. Philadelphia, 1874. (H. Carey Baird.)—All chemists are familiar with Will's Tables, which have introduced multitudes into the mysteries of chemical analysis. Professor Himes has added to this edition two new tables of his own, embodying Bunsen's valuable flame reactions. He has also made valuable additions to the opening chapter on the "Course of Qualitative Analysis," and inserted a new chapter (the last) on the "Detection of Metallic Oxides."

12. *Milk Analyses;* by J. ALFRED WANKLYN, M.R.C.S., &c. New York (Van Nostrand), 1874. 12mo, pp. 73.—This is a practical treatise on the examination of milk and its derivatives, cream, butter and cheese, giving simple and exact methods of analysis, together with the results of the author's own researches in 1871, especially into the milk and butter supplied to the London metropolitan work-houses and hospitals. It is a valuable and timely contribution to an important department of sanitary science. The author has greatly simplified and improved the methods of milk analysis.

13. *Bunsen's Flame Reactions.* pp. 30, 8vo. Also translated by Professor HIMES, and published by Mr. Baird, form a proper supplement to Will's Tables.—This translation of Bunsen's Memoir was originally published in the Journal of the Franklin Institute in 1868.

II. GEOLOGY AND NATURAL HISTORY.

1. *Addition to the Paper,* "*Volcanic Energy: an attempt to develop its true Origin and Cosmical Relations;*"* by ROBERT MALLET. (Abstract.)—Referring to his original paper (Phil. Trans., 1873), the author remarks here that, upon the basis of the heat annually dissipated from our globe being equal to that evolved by the melting of 777 cubic miles of ice at zero to water at the same temperature, and of the experimental data contained in his paper, he had demonstrated, in terms of mean crushed rock, the annual supply of heat derivable from the transformation of the mechanical work of contraction available for volcanic energy, and had also estimated the proportion of that amount of heat necessary to support the annual vulcanicity now active on our globe; but, from the want of necessary data, he had refrained from making any calculation as to what amount in volume of the solid shell of our earth *must* be crushed annually, in order to admit of the shell

* Read June 20, 1872; Phil. Trans. for 1873, p. 147.

following down after the more rapidly contracting nucleus. This calculation he now makes upon the basis of certain allowable suppositions, where the want of data requires such to be made, and for assumed thicknesses of solid shell of 100, 200, 400, and 800 miles respectively.

From the curve of total contraction (plate x, Phil. Trans., part i, 1873) obtained by his experiments on the contraction of slags, he has now deduced partial mean-coefficients of contraction for a reduction in temperature of 1° Fahr., for intervals generally of about 500° for the entire scale, between a temperature somewhat exceeding that of the blast-furnace and that of the atmosphere, or 53° Fahr. And applying the higher of these coefficients to the data of his former paper, and to the suppositions of the present, he has obtained the absolute contraction in volume of the nuclei appertaining to the respective thicknesses of solid shell above stated. In order that the shell may follow down and remain in contact with the contracted nucleus, either its thickness must be increased, its volume remaining constant, or the thickness being constant, a portion of the volume must be extruded. The former supposition is not admissible, as the epoch of mountain-building has apparently ceased; adopting the second, the author calculates the volume of matter that must be crushed and extruded from the shell in order that it may remain in contact with the nucleus. He tabulates these results for the four assumed thicknesses of shell, and shows that the amount of crushed and extruded rock necessary for the heat for the support of existing volcanic action is supplied by that extruded from the shell of between 600 and 800 miles thickness, and that the volume of material, heated or molten, annually blown out from all existing volcanic cones, as estimated in his former paper, could be supplied by the extruded matter from a shell of between 200 and 400 miles in thickness.

On data which seem tolerably reliable the author has further been enabled to calculate, as he believes for the first time, the actual amount of annual contraction of our globe, and to show that if that be assumed constant for the last 5000 years, it would amount to a little more than a reduction of about 3·5 inches on the earth's mean radius. This quantity, mighty as are the effects it produces as the efficient cause of volcanic action, is thus shown to be so small as to elude all direct astronomical observation, and, when viewed in reference to the increase of density due to refrigeration of the material of the shell, to be incapable of producing, during the last 2000 years, any sensible effect upon the length of the day. The author draws various other conclusions, showing the support given by the principal results of this entirely independent investigation to the verisimilitude of the views contained in his previous memoir.—*Pro. Roy. Soc.*, No. 152, 1874.

2. *Metamorphic products from the burning of coal-beds of the Lignitic Tertiary in Dakota and Montana.*—In this paper, published in the Proceedings of the Boston Society of Natural History, Jan., 1874 (vol. xvi), Mr. J. A. Allen describes metamorphosed

beds of clays and sands, accompanied by pumiceous and lava-like materials, looking like true volcanic products, occurring far from any volcanic region and due solely to the burning out of beds of brown coal. The thickness of the altered beds amounts in places to thirty or even fifty feet, but seldom exceeds eight to twelve feet. Generally the overlying clays and sands have been merely hardened and changed in color; but in some places, where the coal bed was thick, the deposit in immediate contact has been more or less fused, and has received occasionally a vitreous porcellanic or vesicular structure, and even the scoriaceous and pumice-like aspect of volcanic products. The lowermost of the burnt series consists of cinders and clinkers much like the residuum left in coal grates from the combustion of ordinary mineral coal.

The region of the Bad Lands, on the Little Missouri, is one of the largest areas of this Lignitic metamorphism, it covering a breadth of twenty to thirty miles for 200 miles in length; all the ridges and buttes are capped or bounded with the reddened and indurated shales. Other such areas occur along the Yellowstone near the mouth of Powder River, and along Powder River, and also on the Rosebud and Tongue Rivers. They were found by Dr. Hayden on the sources of Tongue River, within a few miles of the Big Horn Mountains and on those of North Fork of the Shyenne River and elsewhere; and by Dr. Hines as far south as the "foot slopes" of the same range on the Crazy Woman's Fork of Powder River. Nicollet long since described, but from report, the *pseudo-volcanoes*, or smoking hills of the west, which were made, evidently, from the burning of the subjacent coal beds, and he attributes the fire to the action of decomposing pyrites on lignites and other material of a combustible nature.

Mr. Allen states that the landscape is variously affected by the metamorphism The baked rocks, besides giving their red tints to the country, arrest or greatly retard the erosion of the buttes and ridges consisting of them. Over areas of thousands of square miles they thus in great measure determine the surface contours and protect the hills from rapid denudation. Fragments of pumice have been found on the Missouri as far south as Port Pierre, and the early explorers supposed them to be the products of unknown volcanoes, high up in the mountains.

3. *Lignitic formations north of the parallel of* 49°.—Mr. G. M. Dawson, in the last number of the Canadian Naturalist, and also in a report to the British North American Boundary Commission (Montreal, 1874), gives an account of his own and others observations on the region north of the parallel of 49° and west of the Red River. The boundary line strikes the Lignitic Tertiary about 250 miles west of Red River, in the valley of the Souris River. Few distinct plants were found in this region, but there were shells representing two species of *Paludina* or *Viviparus*, at least two of *Melania*, one *Corbula*, and others resembling those described by Meek from the Lignitic beds farther south. The coal is stated

to be of excellent quality. Scoria and clinkers from the burning of the beds in place are stated to be abundant; but Mr. Dawson says that pyrites cannot have been the source of the combustion, since it is of rare occurrence in the coal. To the west the beds occur on the upper parts of the northwestern tributaries of the Missouri, 345 miles west of Red River and beyond; at 400 miles from Red River, near Porcupine Creek, there are many plants in the beds, as leaves of *Thuja*, *Sequoia*, *Taxus*, *Populus*, *Salix*, *Ulmus*, *Platanus*, etc.; also the fern, *Onoclea sensibilis*, which is very abundant. The best of the brown coal afforded Mr. Dawson 42 to 47½ per cent of fixed carbon, 12 to 17 per cent of water, 32 to 40 per cent of volatile matter, and 2½ to 5 per cent of ash. None of those examined afforded a coherent coke.

In a following paper, Mr. Dawson shows that the Cretaceous at Manitoba contains numerous foraminifers, and the species resemble much those of the English chalk. Both abound in forms allied to Textularia and Rotalia. Various forms of coccoliths were found in the same rocks.

4. *Marine Champlain Deposits on lands north of Lake Superior.*—Dr. Dawson, in his annual address before the Natural History Society of Montreal, May 18th, says that Prof. Bell, in the Report of the Canadian Geological Survey for 1870–71, states the occurrence of marine shells, similar to those of the Champlain deposits in the vicinity of Montreal, at a height of 547 feet above the sea. Dr. Dawson also remarks that in the hills behind Murray Bay and Les Eboulements, he has observed these shells at a height of at least 600 feet; and also that Mr. Kennedy has recently found marine shell deposits of the same era on Montreal Mountain, at a height of 534 feet above the sea.

5. *Climate of the Champlain Period.*—In the address referred to in the preceding article, Dr. Dawson repeats his conclusion that the climate of the period when the land stood below the present level—"that which immediately preceded our own modern age"—was cold. The evidence to which he appeals proves the existence of a cold climate as regards the waters, that is, of cold currents on the coast, but no facts are mentioned which tell anything with regard to the climate over the land.

6. *Fossil Cockroaches from the Carboniferous of Cape Breton.* (Canadian Naturalist, vol. vii, No. 5.)—Mr. Scudder here describes two new species from two wings discovered by R. Brown, Esq. He names them *Blattina Bretonensis* and *B. Heeri*. The specimens are on dark shale, and are associated with leaves of *Sphenophyllum* and ferns.

7. *Fossil Elephant and Mastodon in California.*—According to a letter from Dr. L. G. Yates, of Centreville, Alameda County, California, read by Dr. Leidy before the Academy of Natural Sciences of Philadelphia, (and published in the Proceedings for 1874, pp. 18–26), remains of Elephants have been found in Alameda, Calaveras, Los Angelos, Placer and Solano Counties; and the Mastodon in Alameda, Amador, Calaveras, Contra Costa, El

Dorado, Mendocino, Placer, Santa Barbara, Stanislaus, Solano, Sonoma and Tuolumne Counties. The special localities in each county are mentioned in the letter; and Dr. Yates adds that the majority of them he has himself visited. He observes, that although he has looked for human remains or relics in the same beds, he has failed to discover any. He once found a human skull apparently in the same formation which had afforded elephant and mastodon remains; but he afterward discovered the body to which the head belonged, about eighteen inches below the surface, in the bank above where there had been an old Indian burial ground.

8. *Bulletin of the Cornell University.* (*Science.*) Volume i, number 1 and 2. 64 pp. 8vo, with 9 plates. Ithaca, 1874.—This first scientific Bulletin, from the Cornell University, contains two valuable papers: one, a reconnoissance of the Lower Tapajos, being a preliminary report of the Morgan Expedition to the Amazon in 1870, 1871; by CH. FRED. HARTT; the other, descriptions and figures of the Carboniferous Brachiopoda of Itaituba, on the Rio Tapajos, Province of Para, by O. A. DERBY. The latter paper shows, as stated in its conclusions, that the Carboniferous rocks of South America thus far made knows by their fossils, are true Carboniferous, and not Subcarboniferous; that of the 27 species described, 12 are also North American (these including the common kinds *Athyris subtilita*, *Spirifer cameratus*, *Chonetes glabra*, *Productus cora* and others); that several Brazilian and North American species are included among those described by D'Orbigny from Lake Titicaca and Yarbichambi, Bolivia; and among those from the province of Arque, a degree or more southeast of Lake Titicaca, by Col. Lloyd; and others from Cochabamba and Santa Cruz. Mr. Derby's paper appears to be worked up with great care. It is illustrated by nine photographic plates.

9. *Memoire per servire alla Descrizione della Carta Geologica d'Italia.*—The first volume of the series, in illustration of the geology and geological map of Italy, was published, at Florence, in 1871, and the first part of the second in 1873. They are quarto volumes, beautiful in style of execution, and admirable in the maps and engravings. The first has already been briefly noticed in this Journal. The second contains papers: on the Physical geography of the island of Ischia, by Dr. C. W. C. Fuchs; on the Alpine chain of the St. Gothard to be tunneled for the Italico Helvetic railway, by F. Giordano; on the Tertiary formations of the Sulphur-bearing zone of Sicily, by S. Mottura; Malacologia Pliocenica Italiana, by Dr. C. d'Ancona. Fasc. II, genera *Pisania*, *Ranella*, *Triton*, *Fasciolaria*, *Turbinella*, *Cancellaria*, *Fusus*.

10. *Note on Prof. J. Lawrence Smith's collection of his Memoirs;* by GEORGE J. BRUSH: addressed to the Editors.—My attention has been called to a volume entitled "Mineralogy and Chemistry, Original Researches by Professor J. Lawrence Smith," in which there appears, on page 109, in connection with an article on the "Reëxamination of American Minerals," the following footnote: "In the first half of this reëxamination I was assisted by my friend, George J. Brush." As this statement may lead to misap-

prehension in regard to the relations of Professor Smith and myself on the part of some readers, I desire in this place to give the facts.

The first three papers on the "Reëxamination of American Minerals" were published as a *joint* work by Professor Smith and myself, in volumes xv and xvi in the second series of this Journal. In the volume of researches above alluded to, these cover pages 109 to 141, to the end of the section on petalite; and for the matter therein contained I simply claim to be responsible equally with Professor Smith. This claim can be easily substantiated by reference to the original papers in this Journal, and by other evidence if necessary.

I have no doubt Professor Smith will be glad to have this correction made, and presume it was only an accidental inadvertence which led him to publish the foot-note without further explanation. I should not offer this correction had I not learned that Professor Smith's volume has had a wide circulation, both in this country and in Europe.

New Haven, July 8th, 1874.

11. *Cuarto Appendice al Reino Mineral de Chile i de las Republicas vecinas, publicado en la segunda edicion de la Mineralojia*, de DON IGNACIO DOMEYKO, Rector de la Universidad. 58 pp. 8vo. Santiago, Chile. 1874.—This fourth appendix to the second edition of Domeyko's Chilian Mineralogy contains notes on new localities, with descriptions of various minerals, the most of them metallic species. For a double chlorid of silver and mercury at Los Bordos, in the department of Copiapo, the name Bordosite is given by Señor Bertrand. Ulexite and Hayesite are stated to have been found at a locality on the river Loa in littoral Bolivia, and in Carmen Alto, fourteen leagues from Antofagasta, the old localities being in the desert of Atacama in Peru, and at Ascotan in Bolivia. In addition, Domeyko now adds another locality at Ola, about thirty leagues to the east of the mines of copper of Chañaral de las Animas, northeast of the range of Doña Ines; the place appears like a dried lake. The locality of borocalcite (Hayesite), in the dry lake of Maricunga, fifty-nine miles to the north of Puquios, is, according to Fonseca, of great extent, he estimating the amount at 14,000,000 tons. A memoir on the subject has been published by Fonseca in the Anales de la Universidad. It is mainly a *hydrated borate of lime*,—borocalcite, mixed with some common salt, but without any ulexite (boronatrocalcite).

12. *Veszelyite.*—A new mineral has recently been described by Prof. A. Schrauf, the eminent crystallographer of Vienna, under the name of *Veszelyite.* It is triclinic, resembling distorted liroconite, the crystals being bounded by the prism and dome ($100 : 001 = 101° 3'$). It has a bluish-green color, and the composition is expressed by the formula $4CuO\,P_2O_5\,5H_2O$. It occurs on garnet at Morawitza in Banat. It is named from the discoverer.

13. *On Livingstonite, a new mineral;* by MARIANO BARCENA. (El Minero Mexicano, May, 1874.)—Livingstonite much resembles in color and aspect stibnite or sulphid of antimony. It occurs in

prisms, apparently isomorphous with stibnite, and like it in thin columnar groups. Color, bright lead-gray; of powder red, instead of black like stibnite. Hardness, 2 on Breithaupt's scale. Density, at 16° C, 4·81. Fuses at the first touch of the blowpipe flame, and gives out abundant white fumes. Cold nitric acid does not sensibly attack it; but warm dissolves it and a white residuum falls. Sulphydric acid precipitates it, forming a yellow sulphid and another of a black color. Reactions show that it contains mercury as well as antimony. An analysis has not yet been completed, but an assay proved the presence of 10 per cent of mercury, showing that it is in all probability a sulphid of mercury and antimony.

It is from Huitzuco, in the State of Guerrero, Mexico. Mr. Barcena has named it in honor of the distinguished African traveler, Mr. Livingstone; with reference to which, he well says, "Al hacer esta dedicatoria he tenido presente que los bienhechores de la humanidad pertenecen á todas las naciones, y que la humanidad entera debe honrar su memoria."

14. *On the Plagopterinæ and the Ichthyology of Utah;* by E. D. COPE. 14 pp. 8vo. 1874.—A part of the Report of the Geographical and Geological Explorations and Surveys west of the 100th meridian; First Lieut. G. M. Wheeler, Corps of Engineers, U. S. A., in charge. From the Proceedings of the Amer. Phil. Soc. of Philadelphia.

15. *Annotated List of the Birds of Utah;* by H. W. HENSHAW. 16 pp. 8vo. (Reprinted from the Ann. N. York Lyc. Nat. Hist., vol. xi, 1874, at Salem, Mass.)—Mr. Henshaw's personal observations were made in connection with the survey under Lieut. Wheeler, of which he is ornithologist.

16. *Bulletin of the Buffalo Society of Natural Sciences.*—Vol. ii, No. 1 (104 pp. 8vo), contains a list of the Noctuidæ of North America, by A. R. Grote; a catalogue of the Coleoptera from the region of Lake Pontchartrain, Louisiana, by S. V. Summers; and a catalogue of Boleti of New England, with descriptions of new species, by C. C. Frost.

17. *Anatomy of the Invertebrata;* by C. W. VON SIEBOLD. Translated from the German, with additions and notes, by Waldo I. Burnett, M.D. 470 pp. 8vo. Boston, 1874. (James Campbell.)—This is a reprint of Dr. Burnett's excellent translation of Von Siebold's well known text-book on the Anatomy of the Invertebrata. It is a standard work, and, although not presenting the latest results of researches, should be in the hands of all zoological students. The notes of Dr. Burnett are an important addition to the original work.

18. *Maps of the Geyser Basins, on Madison River*, after a reconnaissance by G. R. BELCHER. U. S. Geological Survey of the Territories, F. V. Hayden in charge.—These two large maps give the exact positions and areas of the many hot springs, or lakes and geysers, of the Upper Madison. They are a most interesting study for the geologist. Dr. Hayden has the satisfaction of seeing great and valuable results flowing from the explorations under his charge.

19. Three Essays relating to Vegetable Paleontology are before us:

(1.) ALPH. DECANDOLLE, *on Physiological Groups in the Vegetable Kingdom: (Constistution dans le Règne Végétal de Groupes Physiologiques applicables a la Geographie Botanique Ancienne et Moderne)*; par M. ALPHONSE DECANDOLLE.—An article of 38 pages in the Archives des Sciences de la Bibliothèque Universelle, Geneva, of May, 1874, and issued separately. M. DeCandolle here makes an attempt to mark out groups of plants according to the climates which they affect.

From the grouping of plants according to their botanical characteristics, he says, come classes, families, genera, species, &c. From geographical delimitations come floras. For the study of geographical botany and of the vegetation of former periods, some kind of physiological arrangement, which may comport with constitutional characteristics and aptitudes, becomes desirable; and this want M. DeCandolle here undertakes to supply.

He sketches five groups. First, plants requiring a great amount of heat and moisture. A name expressive of both these requirements would be cumbrous; so he chooses for this group a name referring to the temperature only, and calls them *Megathermal* plants, or in short *Megatherms*, i. e., plants to which much heat is essential. These inhabit the rainy intertropical regions in the plains, and sultry valleys up to the 30th parallel.

Second, plants requiring about as much heat but far less moisture. These, taking the name from the latter characteristic, he terms *Xerophilous* plants, lovers of dryness. They are pretty widely distributed, but they especially affect the regions bordering the tropics and extending, say to the 35th parallel in both hemispheres.

The third group, *Mesothermal* plants, require, as the name denotes, moderate heat, also a fair supply of moisture, at least in the growing season, say a mean annual temperature of 59°–68° Fahr., that of the rather warm temperate zones.

The fourth group, *Microthermal* plants, i. e., those demanding little heat, say a mean annual temperature of 57° or less, down to that of 32°, with of course a good summer temperature. In this group would be included the vegetation of our Northern States and Canada.

The fifth group, *Hekistothermal* plants, those requiring least heat, such as make up arctic, antarctic, and alpine vegetation.

His sixth group, *Megistothermal* plants, those which require an exceptional amount of heat, or a mean of over 86° Fahr., are mentioned as having probably played a part among the earlier vegetations, but as now represented only by a few lowly organized plants of thermal waters.

The present distribution of plants over the globe depends, 1, upon the distribution in a preceding epoch, and 2, upon the physical conditions, mainly the distribution of heat and moisture, now existing. These conditions have varied greatly; and under them

the corresponding sorts of plants have occupied stations widely different from those they now occupy. As illustrating this, DeCandolle mentions the fact that the vegetation of the borders of the Mediterranean extended to Paris at the commencement of the present period; that the arctic-alpine or hekistothermal vegetation, now belonging to the polar region and the summit of high mountains in Europe, occupied the plains in the glacial period after having been once before stationed nearly as at present; that the vegetation, say of the 35th parallel in the Atlantic United States, has once been as far north as the 60th parallel; and the intertropical flora had advanced to London at the commencement of the Tertiary period. In another connection he remarks that, "il est à peu près démontré que les flores actuelles des Etats-Unis méridionaux et du Japon se sont rapprochées un fois dans le nord, sous l'empire de climats tempérés, selon l'hypothèse émise, en 1859, par M. Asa Gray." Now when one endeavors to trace the probable connection between preceding and present vegetations, as it were historically, it becomes convenient, if not almost necessary, to have some name under which a particular *ensemble* of vegetation, such as that of a large part of the United States in our time, may be spoken of as a whole when recognized in far other times and places. Hence these proposed names, of Mesotherms, &c. If the grouping and the naming, as now struck out, are not wholly satisfactory, at least a fair beginning has been made and a practical convenience secured. Their utility becomes apparent when attention is turned to fossil floras.

In these the dominant character, megathermous, mesothermous, microthermous, or even xerophilous, is pretty clearly to be made out, in the Tertiary floras unmistakably. That similar forms of vegetation at different eras imply similar climates is an unavoidable hypothesis; that they imply similar antecedent plants is the natural presumption; and when, along with the general similarity, some of the species of consecutive floras appear to be identical or nearly so, this presumption approaches demonstration. In following up this line in the regions and through the times which we know most about, viz: in Europe and North America, and from the present to the Cretaceous period, DeCandolle brings to view various interesting considerations, and among them a corollary drawn by Heer, which our paleontological geologists should heed. Namely, that when two fossil floras or faunas are very similar, but in widely separated latitudes (say Spitzbergen and middle Europe for a marked instance), they cannot possibly have been contemporaneous. For difference of temperature according to latitude must have been as real as now within the periods referred to, however modified by geographical configuration; and of two such floras the northern must have been the more ancient, since the temperature has on the whole diminished in the course of ages, and especially during the Tertiary epochs. And not only very different fossil Tertiary floras must have been contemporaneous in different latitudes, but probably under the same parallels

also, at least as different as the present floras of Europe and China, or of California and Pennsylvania.

How the adaptive diversification into these physiological groups came to pass, is a question of the same nature as that of the diversification into classes, genera and species, but far easier to form a conception of. As a specimen of the essay we translate a portion of DeCandolle's remarks upon this:

"Starting from the idea, admitted by physicists, of a nearly uniform and considerably elevated temperature of the earth at a remote period, followed by a very gradual cooling and distinction of climates during an incalculable series of ages, there must have been at first only one physiological group of plants, that which I have called megistothermous. To this succeeded the plants of the Carboniferous era, of little diversification, however, as compared with those of subsequent ages. They would seem to have been megatherms, or sometimes mesotherms, for the existing ferns and conifers correspond to those two categories. Among them there may have been species adapted to the long polar twilights, just as our ferns often inhabit the forests, and as some cultivated conifers, such as *Cryptomeria Japonica*, prefer the shade. The snow must have destroyed the primeval vegetation of the north, but the megatherms and mesotherms survived elsewhere. It is difficult to conceive what was passing during the immense period of the secondary formations, on account of the subsequent dispersion of the land surfaces, the extent of the seas which covered Europe, and the fewness of the fossil plants which have thus far been studied. At the commencement of the Tertiary period, the megatherms occupied all the exposed surface up to the 48th degree of north latitude, that is to say, all the present hot and temperate region. The other classes became detached little by little from the vegetation that preceded, and localized toward the north and upon the mountains, in proportion as the increased cold drove out the prior possessors. Gradually the megatherms lost territory and the others acquired it. This is the simple expression of the facts, irrespective of theory.

"How and by what means has this physiological grouping in accordance with the climates been effected? Here hypothesis begins. The question is the same as that which respects the evolution of forms, except that here there is a basis of known facts which in that case is wanting; and nobody can prove that there was originally only one kind of plant at a time when there was certainly only one climate. And unity of climate carries with it physiological unity for the vegetation of the epoch, whatever it may have been. There is nothing hypothetical therefore in saying that the actual physiological groups have succeeded to the single primary one. As to the cause of this evolution of groups, the hypothesis proposed by Darwin for that of forms is equally applicable, and equally finds support in the circumstances as they become more and more known. Thus the changes both in physiological character and in the forms have been very slow and very

gradual. The study of the Tertiary floras, as pursued by M. de Saporta, following in the footsteps of Heer, furnishes abundant proofs of a slow and continual substitution of analogous forms, which is quite contrary to the hypothesis of abrupt renewals at long intervals, through unknown causes, as maintained by the eminent Zurich professor. But I need not now discuss the Darwinian hypothesis. At least we may wait until some other discussable hypothesis is propounded."

As we write this, the information comes to this country that M. DeCandolle has the distinguished honor of being elected one of the eight foreign members of the Institute of France (Academy of Sciences), to fill the vacancy made by the death of Agassiz.

(2.) W. C. WILLIAMSON: *Primeval Vegetation in its relation to the Doctrines of Natural Selection and Evolution.*—This is one of the fourteen able *Essays and Addresses by Professors and Lecturers of Owens College, Manchester*, published in commemoration of the opening of the new college buildings, Oct. 7, 1873 (Macmillan & Co., London, 1874), and forming a handsome octavo volume of 560 pages. Professor Williamson's lecture fills fifty-five pages, with a sketch of what is known of the vegetation of the preceding, mainly of the earlier, geological periods, and with evidently well considered and noteworthy remarks upon the bearings of our actual knowledge on the question referred to. It opens with a reference to the "wide-spread commotion in the scientific world" caused by "the allied doctrines of Darwin and Herbert Spencer in relation to the origin of species." The popular association of these names is natural enough, and Mr. Darwin has himself to some extent furthered it. But scientific men, it is to be hoped, keep up a distinction between natural history investigation pursued by evolutionary hypotheses and the *a priori* deductive natural history developed by Mr. Spencer. The former brings new aids to research and practical problems to be attempted: of the latter it is unnecessary now to speak, and we could not well speak of it so highly as Professor Williamson does. On the other hand Professor Williamson alludes to "the advocates of the new views" as "treating with ill-concealed contempt any one belonging to an older scientific school who is unable to travel along the new road with their vigorous steps." Surely this cannot be justly said of any who have treated of vegetation, recent or primeval, under the light of the "new views," nor of Mr. Darwin himself; indeed, this is so far from being true that it is fair to presume that the professor had some other order of persons in his mind. He concludes that "the geological record is less imperfect botanically than zoologically, since we practically catch the vegetable kingdom at a comparatively early stage of its history, and are able in some measure to trace its upward progress,"—propositions which, we suppose, are open to serious doubt. That "the extinct vegetable kingdom has been comparatively neglected in connexion with this subject" may be true; but a singular idea of what has been done and attempted is conveyed when Dr. Dawson's name alone is mentioned in this connexion, and Heer and Saporta, New-

berry and Lesquereux, are left out of view—perhaps because they have not written polemically. These are all the faults we have to find with what appears to be a fair and faithful presentation of the topic, as respects the fossil botany of the Devonian and Carboniferous formations, with which the author is thoroughly familiar, and to which he mainly restricts his attention in this essay. His conclusions are that natural selection and evolution "unquestionably account in a satisfactory manner for many phenomena that have hitherto received no other adequate explanation," such, for instance, as the production of varieties, "while it is equally certain that many of these varieties are wholly undistinguishable from what we designate [as] species; it is also true that in many instances generic types appear to merge into each other in such a way as to make it difficult to define their boundaries." The longer he studies any group and the more numerous the specimens obtained from different localities, the more utterly does he distrust their specific and even generic distinctions; and he can account for this state of the case only on the supposition of "intermediate forms produced through the action of the varying external forces which lead to evolution." To the question whether variations are boundless or are confined within definite limits, while admitting that those limits may include genera, perhaps he would include families also, he replies that "the facts of paleophytology appear to me to favor the latter rather than the former conclusion. We have not much difficulty in distinguishing the great paleozoic types from one another." This, surely, is what would be expected, at least by Mr. Darwin.

(3.) J. W. Dawson: *Annual Address of the President of the Natural History Society of Montreal*, May, 1874.—A considerable portion of this address is devoted to vegetable paleontology and related matters. Substantially Principal Dawson's views and aims are not very unlike those of Prof. Williamson, although the mode of argument and the tone are. Both have occupied themselves mainly with the fossil botany of the earlier formations; and this may explain the contrast between their conclusions as to the abrupt introduction of types and forms, and those of Count Saporta, based upon the fuller records of Tertiary deposits, which DeCandolle has referred to in an extract given above. Professor Williamson probably thinks that evolutionary hypotheses in some form are not unlikely to prevail; Dr. Dawson, that they are already well nigh exploded. The former seems inclined to accept at least the probability of evolution for species and genera. Dr. Dawson restricts its sway "to varietal and race forms," which "constitute conventional as distinguished from natural species," maintaining that the latter are permanent and original. But, so far as we can make out, the difference between his "varietal forms or conventional species," which have arisen from "descent with modification," and the natural species which have not, is simply this: that actual evidence (such as that of known intermediate forms) has enforced the conclusion in certain cases and not in others. Whenever it does, or tends to do so, we have only to transfer these from

the list of actual to that of conventional or spurious species; and so the doctrine of the "marvelous permanence of specific types" will remain unassailable, while the truths for which "modern theories of derivation have been chiefly valuable, so far as established, will remain as substantial results after these theories have been exploded." "Exploded" is hardly the proper term (except as suggestive of the "engineer hoist with his own petard,") for a process in which the doctrine of the derivation of species has all the life taken out of it as fast as it comes into existence. This method of confutation is more effectual than original. It was foreshadowed by Sir John Harrington in the 16th century; when he wrote:—

> "Treason doth never prosper, what's the reason?
> "Why, if it prosper, none dare call it treason.

This absolute distinction into varietal and derived as against specific forms assumed to be underived, along with the recognition that some of the latter have come down through the Tertiary or even from the Cretaceous ages, "while some species have disappeared without known successors, and others [like them?] have come in without known predecessors," seems to be the key to the interpretation of some points in the following passage. We quote it at length, being doubtful if we could do it justice in an abstract.

"Physically the change from the Cretaceous to the Tertiary was one of continental elevation—drying up the oceanic waters in which the marine animals of the Cretaceous lived, and affording constantly increasing scope for land animals and plants. Thus it must have happened that the marine Cretaceous animals disappeared first from the high lands and lingered longest in the valleys, while the life of the Tertiary came on first in the hills and was more tardily introduced on the plains. Hence it has arisen that many beds which Meek and Cope regard as Cretaceous on the evidence of animal fossils, Newberry and Lesquereux regard as Tertiary on the evidence of fossil plants. This depends on the general law that in times of continental elevation newer productions of the land are mixed with more antique inhabitants of the sea; while on the contrary in times of subsidence older land creatures are liable to be mixed with newer products of the sea. Thus in Vancouver's Island plants which Heer at first regarded as Miocene have been washed down into waters in which Cretaceous shell-fishes still swarmed. Thus Cope maintains that the lignite-bearing or Fort Union group contains remains of Cretaceous reptiles, while to the fossil botanist its plants appear to be unquestionably Tertiary. Hence also we are told that the skeleton of a Cretaceous Dinosaur has been found stuffed with leaves which Lesquereux regards as Eocene. At first these apparent anachronisms seem puzzling, and they interfere much with arbitrary classifications. Still they are perfectly natural, and to be expected where a true geological transition occurs. They afford, moreover, an opportunity of settling the question whether the introduction

of living things is a slow and gradual evolution of new types by descent with modification, or whether, according to the law so ably illustrated by Barrande in the case of the Cephalopods and Trilobites, new forms are introduced abundantly and in perfection at once. The physical change was apparently of the most gradual character. Was it so with the organic change? That it was not is apparent from the fact that both Dr. Asa Gray and Mr. Cope, who try to press this transition into the service of evolution, are obliged in the last resort to admit that the new flora and fauna must have migrated into the region from some other place. Gray seems to think that the plants came from the north, Cope supposes the mammals came from the south; but whether they were landed from one of Sir William Thomson's meteors, or produced in some as yet unknown region of the earth, they cannot inform us. Neither seems to consider that if giant Sequoias and Dicotyledonous trees and large herbivorous Mammalia arose in the Cretaceous or early Tertiary, and have continued substantially unimproved ever since, they must have existed somewhere for periods far greater than that which intervenes between the Cretaceous and the present time, in order to give them time to be evolved from inferior types; and that we thus only push back the difficulty of their origin, with the additional disadvantage of having to admit a most portentous and fatal imperfection in our geological record." (pp. 9, 10.)

The latter portion of this paragraph conveys a singular idea of what one of the writers referred to by Principal Dawson was about. Dr. Gray had a problem before him, of which these are the salient points and the emphatic facts: The *Taxodium* or Bald Cypress of our Southern Atlantic States, or a tree so like it as to be now undistinguishable, was in the Miocene age an inhabitant of the arctic regions round the world. The Sequoias or Redwoods of California were represented in this same arctic Miocene flora by several species of similar trees, two of which were so much like the two present Californian Redwoods that, if we mistake not, Dr. Dawson is disposed to regard them as "varietal forms" of the same. Besides these peculiar types, the arctic Miocene forest contained, and apparently consisted of, trees and shrubs generically like those which compose the forests of New England, Virginia, etc., some of them generally resembling, others very closely resembling, and some seemingly specifically representing, our existing trees. What was the probable relation between these existing trees of our latitude and the arctic ones of ancient times? In answering this question, Dr. Gray took two postulates for granted, in common with the scientific world: 1, that the more recent individuals of a species are descendants of the earlier individuals of the same species; 2, that the temperate-climate vegetation which once flourished beyond the arctic circle was slowly driven southward by the cold. His moderate theoretical inference is, that the vegetation among which we live has, as a whole, descended from the arctic Miocene vegetation, most of it with modification, some of it without. This is what Dr. Dawson calls being "obliged in the

last resort to admit that the new flora and fauna must have migrated into the region from some other place. Gray seems to think that the plants came from the north, etc." Why, instead of the last, it is the first resort. Surely, unless astronomical conditions have been reduced, the plants in question were not growing here at the time when their analogues, some even of the same species, were flourishing beyond the arctic circle; and Dr. Gray might naturally *seem to think* that they came from the north, since they could not stay there any longer, and that is where they were known to be at the last previous account. Dr. Dawson intimates that they may as well have "landed from one of Sir Wm. Thomson's meteors." DeCandolle, as quoted above, asserts that the view which Dr. Dawson thinks wholly fanciful, is all but demonstrated.

Finally, the writer referred to, following Lesquereux, intimates that the traces of the same genera and of similar species in the Cretaceous suggest a descent of our principal types from that remoter period. This supposition ought not to appear very violent to one who says of a common Canadian Fern, *Onoclea sensibilis*, "There is every reason to believe that this is merely one example out of many, of plants that were once spread over Europe and America, and have come down to us unmodified throughout all the vicissitudes of the Tertiary ages." (p. 11.) One would fain suppose that if so many species have come down all this way "unmodified," and when "we have learned that each specific type has capacities for the production of varietal and race forms, which are usually exercised to the utmost in the early stages of its existence, and then remain fixed or disappear and reappear as circumstances may arise" (p. 14), then it is not unlikely that still more have come down in modified forms. The problem was, to trace the connection between the present temperate flora and that of the later Tertiary, and remotely with its predecessors, as far back and to such a degree as the diminishing evidence might allow. Upon Dr. Dawson's own data and principles an attempt of the kind would seem to be perfectly legitimate. But the close of the long paragraph quoted above seems to imply that the immediate deductions from admitted facts and the remoter inferences are equally vain; that it is useless to derive our temperate flora even from that of the Miocene, or that from the Eocene, or that from the Cretaceous, until we are able to show how and whence it came to the Cretaceous, and so on. As if, when one offered to prove, by appropriate testimony, that Dr. Dawson came to Montreal from Nova Scotia, and his parents or grandparents came to Nova Scotia from Great Britain, he were told that this was of no account, until it were shown how and when their ancestors came to Great Britain. As if the object in endeavors of this sort was to show how things began. The practical questions relate to how they go on, and especially how they have gone on within times respecting which there is much evidence to be had.

What view does Dr. Dawson himself take of the relation between the North American flora and that of the Tertiary ages?

Many species, he believes, are common to the two. "But while this is the case, some species have disappeared without known successors, and others have come in without known predecessors. Nay, whole floras have come in without known origin. Since the Miocene age the great Arctic flora has spread itself all around the globe, the distinctive flora of Northeastern America and that of Europe have made their appearance, and the great Miocene flora, once almost universal in the northern hemisphere, has as a whole been restricted to a narrow area in Western and warm temperate North America. Even if with Gray, in his address of two years ago before the American Association, we are to take for granted that the giant pines (Sequoias) of California are modified descendants of those which flourished all over America and Europe in the Miocene, Eocene and Cretaceous, we have in these merely an exceptional case to set against the broad general facts. Even this exception fails of evolutionary significance, when we consider that the two species of Sequoia, which have been taken as special examples, are at best merely survivors of many or several species known in the Cretaceous and Tertiary. The process of selection here has been merely the dropping out of some out of several species of unknown origin, and the survival in a very limited area of two, which are even now probably verging on extinction; in other words, the two extant species of Sequoia may have continued unchanged except varietally from Mesozoic times, and other species existed then and since which have disappeared; but as to how any of them began to exist we known nothing, except that, for some mysterious reason, there were more numerous and far more widely distributed species in the early days of the group than now."

That is, "the facts have no evolutionary significance," because the species which are identical (such as the *Taxodium* and *Onoclea*) have come down unchanged, and so testify to the fixity of species; those that may possibly be "modified descendants," such as the two Sequoias, survive only in a very limited area and are therefore of no account; while those that are specifically different in the two floras "have come in without known origin," to take the place of others, in numerous cases extremely like them, which "have disappeared without known successors." The singular resemblance between the two is an inexplicable mystery to Dr. Dawson, excepting when, now and then, it is seen to be a trifle closer, which explains that particular instance, but to him throws no ray of light on the rest.

We have no idea whose reasoning it is that is so contemptuously spoken of on the 13th page of Dr. Dawson's address. Probably it never occurred to his mind that other people might find fault both with the substance and the style of his own argumentation. A. G.

Robert Shuttleworth *and his Collections.*—Mr. Shuttleworth was an English botanist, long domiciled in Switzerland, where he happily married. He was of no small scientific note, although he published very little, and his collections are ample and important.

His death followed close upon that of his old and near friend Meissner, of Basel. His herbarium, we understand, is in part bequeathed to certain institutions and individuals, and in part will be sold. His library, said to be very rich in other branches of natural history, as well as in botany, is by the late owner's desire to be sold entire. This will afford an excellent opportunity for some of our colleges or other institutions. No particulars have as yet been received; but any institution wishing to secure a valuable library, such as it takes a long and devoted life to form and perfect, should address Madame Shuttleworth, at Berne, Switzerland. A. G.

III. Astronomy.

1. *Polariscopic Observations of Coggia's Comet* (1874, III).—During the greater portion of the time when the comet was favorably situated for observation, it was unfortunately hidden by clouds, or extinguished by thick haze. On one or two occasions, however, I was able to obtain evidence that its light was polarized. The instrument employed was the very sensitive polariscope described in the May number of this Journal.

On the evening of July 6, although the sky was generally full of drifting clouds, clear intervals appeared now and then, which allowed a distinct view of the comet. The polariscope showed the bands, both bright and dark, quite definitely, and they were seen with comparative ease. Observations repeated a number of times agreed in showing that the light was polarized in a plane passing through the axis of the tail, that is, as nearly as could be estimated, in a plane passing through it and the sun. Other observations made on the evening of July 14, when the sky was quite clear, gave the same result, though less satisfactorily, as the twilight had begun to interfere with the observations. After waiting until this had disappeared, it was possible to see the bands, though with some difficulty, and the degree of the polarization appeared to be decidedly less than on the previous occasion.

The circumstances were too unfavorable to admit of any determination of the percentage of light polarized, but it was certainly not large. The fact of polarization shows that a considerable portion of the light of the coma is derived from the sun by reflection. A. W. WRIGHT.

Yale College, July 24, 1874.

2. *Spectrum of Coggia's Comet* (1874, III).—In the Comptes Rendus for June 8, 1874, M. Rayet gives an account of spectroscopic observations, made early in June, which showed that the spectrum of the nucleus was continuous, resembling that of a star of the sixth magnitude, except that the color at the extremities could not be distinguished. The spectrum of the coma consisted of the usual three bands of cometary spectra, differing from them, however, in the fact that instead of being sharply terminated on the less refrangible side and fading gradually away toward the blue, they were definitely bounded on both sides, the central band especially terminating sharply on both edges as if by a line,

while the other two were slightly diffuse on both edges, thus approaching the ordinary type. They were less than half as bright as the central band. M. Rayet states that the three corresponded respectively with the yellow, green and blue parts of the spectrum, without giving more definitely their exact positions and dimensions. He speaks of the nuclear spectrum as very short. The same peculiarity was observed by Mr. Lockyer, who says (Nature, June 25, 1874), "It struck me that the spectrum was short, i. e., that it was deficient in blue rays; and as one saw in the telescope a fan-like structure above the nucleus (as seen in an inverting telescope), so also in the spectroscope, the continuous spectrum sparkled as if many short, bright lines or bands were superposed upon it."

A. W. W.

3. *Observations of the Aurora Borealis made by* MINERVA E. WING, *at West Charlotte, Vt., lat.* 44° 19′, *long.* 73° 15′ *W. from Greenwich.*—The following observations were communicated to the Smithsonian Institution and have been forwarded to this Journal by Prof. Joseph Henry.

1872. Oct. 5 and 6, Aurora first observed at midnight; lasted until daylight; confined to a belt from W. to E. across the zenith. Oct. 28, 8 P. M., auroral lights low in the north; brightest near midnight. Oct. 29, 8 P. M., a low rim of bright light on the north horizon, threw up a few beams a little E. of N. Occasional auroral lights during the remainder of night.

Dec. 23, 8 P. M., Auroral lights lasted until midnight. Dec. 31, at times quite light in north.

1873. Jan. 5, Aurora at 2 A. M., brightest at 3 A. M., with a few streamers in N. E. Jan. 7, 8½ P. M., aurora in E. and W. sending beams to zenith. Jan. 26, 7½ P. M., aurora till 9 P. M., when it took the form of a low arch on the N. sky. Jan. 29, midnight, aurora similar to that of the 26th.

Feb. 4, 8 P. M., Appearance of a white cloud through which the stars shone, retained the same position all night till 4 A. M. Feb. 20, auroral lights very bright from midnight until morning. Feb. 20, 8 P. M., similar to aurora of Jan. 26th. Feb. 22, 8 P. M., aurora very white; at midnight very bright. Feb. 24, 8 P. M., bright; very white. Feb. 27, aurora from midnight till morning.

March 1, Aurora from 7½ P. M. to midnight. March 5, aurora from 9 P. M. March 10, 8 P. M., a few beams of aurora visible. March 23, faint aurora in north, at 9 P. M. March 24, 8 P. M., bright auroral light in north. March 27, 8 P. M., quite bright in N. W., lasted all night.

April 8, 7 P. M., Crimson aurora. April 11, at midnight red light on N. horizon. April 16, light on N. horizon. April 21, strong auroral light 10 P. M. April 29, faint aurora. April 30, faint aurora.

May 3, Faint aurora all night. May 6, Bright aurora, lasted all night. May 8, aurora all night. May 20, aurora all night. May 22, bright aurora from dark cloud bank in N. May 31, aurora all night.

June 18, Bright display of auroral lights. June 22, auroral lights during the night. June 23, aurora all night. June 25, most brilliant aurora of this year; form of an arch, with short beams playing on both sides up and down; very bright at 11 P. M., 1 A. M. and 3 A. M. June 27, active aurora, 10½ P. M.; beams shot past the zenith.

Aug. 14, Aurora. Aug. 16, arch low on N. horizon. Aug. 17, aurora near horizon from 9 P. M. to midnight. Aug. 19, aurora 10 P. M. in N., a few faint beams. Aug. 27, bright aurora 1 A. M. Aug. 30, Low aurora 2 A. M.

Sept. 3, Bright aurora in N. horizon. Sept. 20, bright auroral light on N. horizon. Sept. 26, aurora from midnight till morning. Sept. 30, bright but low aurora 3 A. M.

Oct. 22, Aurora at midnight.

IV. Miscellaneous Scientific Intelligence.

1. *Smithsonian Report for* 1872. 456 pp. 8vo. 1873. (Government Printing Office.)—This is the twenty-seventh of the series of annual Reports of the Smithsonian Institution. The Report appears more than a year after its date; such are the delays at the Government Printing Office. The Report of the Secretary, Prof. Henry, for the year 1872, covers fifty pages of this volume. In it the line of policy which has been steadily followed during a quarter of a century in the government of this institution, under the will of Smithson (of which the United States act only as trustees), is fully set forth, and its essential wisdom shown by its good results. With an income of only about $45,000, by the use of great economy, the Smithsonian conducts a world-wide system of exchanges of books and objects of natural history, having on its lists the names of nearly 3,000 correspondents in all countries where science is cultivated, to whom its publications are regularly sent, and from whom returns reach its archives. The Smithsonian Contributions (quarto) have now reached seventeen ponderous volumes, filled with original memoirs. Its Miscellaneous Contributions (8vo) are composed of (mostly) original memoirs on natural history, meteorology, and other departments of science designed to aid those engaged in original work. These objects, with the custody of its collections, the care of its buildings, and the payment of its staff, required a wise and frugal administration of its funds to avoid the shoals of insolvency. Under the conduct of its distinguished Secretary, and his Assistant Secretary, Prof. Baird, the Smithsonian Institution has attained a position of commanding influence for good in matters of science. A General Index of these annual Reports at the close of the thirtieth volume (that for 1875) would add greatly to their value.

2. *Seventh Annual Report of the Trustees of the Peabody Museum of American Archæology and Ethnology.* 42 pp. 8vo. Cambridge, 1874.—The Report opens with an account of the Agassiz collection obtained during the voyage of the Hassler, with descriptions of the crania and other specimens. One microcephalic skull, from Ancon, of an individual not quite adult, has the internal capacity 33 cubic inches, or 44 per cent of the average Peruvian cranium, and much smaller than the crania of some Peruvian children not over seven years of age. Though probably idiotic, there are no marked signs of it. The closing article is on Human Remains in the Shell Heaps of the St. John River, East Florida, in which the author, Prof. Wyman, presents reasons for believing that cannibalism was practised by the Indians. In evidence of this, it is stated that the bones were not deposited as in ordinary burial, but scattered in a disorderly manner; secondly, they were broken, as if reduced to a size suitable for the vessels used in cooking; thirdly, there was a degree of method in the breaking of them, showing that it was not done by wild animals.

3. *Geological Survey of Hokkaido: Yesso Coals. A Report by* Henry S. Munroe, E.M. 39 pp. 12mo. Tokei. 1874. Pub-

lished by the Kaita Kushi.—This handsomely printed report (in English) reaches us through Mr. B. S. Lyman, chief geologist and mining engineer. Mr. Munroe has made thorough work of the Yesso coals, giving in a series of tables both proximate and ultimate analyses, and, with the latter, a calculation of the calorific power, evaporative power, and temperature of combustion. The geological horizon of the Yesso coals is not given, but we infer from the following remark by Mr. Munroe that they are probably of Tertiary age. "It will be seen on inspecting this table" (comparative table of coals from different parts of the world) "that these Japanese coals are widely removed by this composition from all coals of similar age, and can be compared with the best Carboniferous coals. They are therefore neither *lignites* nor *brown coals*, but true bituminous coals." This statement the analyses given fully sustains.

Mr. Lyman states the whole thickness of workable and probably good coal, in the Kayanoma field to be at least thirty-five feet, chiefly in beds of from three to seven and a half feet thick.

4. *Suppléments aux Notes sur les Tremblements de Terre ressentis de* 1840 *a* 1868; pp. 70, Bruxelles, 1873; and *Note sur les Tremblements de Terre en* 1870 *avec Supplément pour* 1869; par M. ALEXIS PERREY, Professeur Honoraire a les Facultés des Sciences de Dijon; pp. 146, Bruxelles, 1874.—These are papers presented by Prof. Perrey to the Royal Academy of Belgium, and published in volumes xxiii and xxiv respectively of their *Memoires*. They bring down his published records to the close of 1870. The former adds many notices to those previously published for the years 1843–1868. In the latter, the supplement for 1869 occupies 60 of the 146 pages. In connection with a slight trembling reported at Pulkova, Sept. 20, 1867, the author notices several observations of slight earthquakes made known only by the unusual oscillation of the levels of astronomical instruments.

C. G. R.

5. *Blue Pigment of the Egyptians.*—Fifteen centuries before the Christian era, the Egyptians appear to have been acquainted with the preparation of three distinct kinds of blue pigment, prepared from mixtures of sand, soda, and lime, with oxide of copper. One of these fine colors has been lately examined by M. Henri de Fontenay, who contributes a paper on the subject to the June number of the *Annales de Chimie*. The investigation was conducted in Peligot's laboratory, at the Conservatoire des Arts et Métiers, and some examples of the blue frit were then made at the National Porcelain Factory at Sèvres, under M. Salvétat. The author publishes not only analyses of ancient specimens, but recipes for their imitation. A mixture of 70 parts of white sand, 25 of chalk, 15 of oxide of copper, and 6 of dry carbonate of soda, yielded, when fritted together, a blue material said to be equal in color, texture, and durability to the ancient examples.—*Athenæum*, June 20.

6. *Magnetic Observatory in China*—A magnetic observatory has been established at Zi-ka-Wei, in China, under the superintend-

ence of Father Dechevrens. A first Report of the diurnal variation and of the magnetic intensity for part of March and April last, has been received in this country, from which we gather that the mean of the declination is 1° 53′ 59·8″ W., of the inclination, 46° 13′ 13·7″, and of the intensity, 6·92833. A complete set of self-recording magnetographs, by Adie, has been forwarded to this new observatory; and as Father Dechevrens and his assistant have been well instructed in their use, we may hope that a series of valuable observations of the phenomena of magnetism will be obtained.—*Athenæum*, June 20.

7. *Festschrift zur Feier des hundertjährigen Bestehens der Gesellschaft Naturforschender Freunde zu Berlin.* 264 pp. 4to.—Contains papers presented at the centennial of this Society, July 9th, 1873. The authors are Ehrenberg (on the phosphorescence of the ocean), Rammelsberg (on the chemical nature of some minerals), Rose (on the meteoric iron of Equique), Gerstæcher (on the morphology of *Orthoptera amphibiotica*), Dr. Gustav Fritsch, L. Kny (on the axillary buds of the Florideæ), P. Magnus, E. v. Martens, W. C. H. Peters (on Dinomys, from Peru), Dr. P. Ascherson and Otto Müller. The volume is illustrated by twenty plates.

8. *American Association for the Advancement of Science.*—The twenty-third meeting opens at Hartford, Connecticut, on Wednesday, the 12th of August, at 10 o'clock. Members should report on arrival at the State House, where will be the headquarters of the Association.

9. *French Association for the Advancement of Science.*—The French Association for the Advancement of the Sciences is to hold its third meeting this year at Lille, the session commencing on the 20th, and closing on the 27th, of August. A local committee has organized scientific excursions, under the direction of M. Kuhlmann, corresponding member of the Academy of Sciences, assisted by the mayor of the town and other eminent persons. The President of the Association for the present year is M. Ad. Wurtz.—*Athenæum*, June 20.

10. *Academy of Sciences, Paris.* — A. DeCandolle has been elected Foreign Associate in place of Professor Agassiz. The foreign members of the Academy of this highest grade are entitled to take part in all elections, and enjoy all the privileges of the members in Paris. M. von Kokscharow, of St. Petersburg, has been made Corresponding Member, in place of Professor Sedgwick.

11. *University of Oxford.*—The chair of Geology at Oxford, made vacant by the death of Professor Phillips, has been well filled by the appointment of Mr. Joseph Prestwich, F.R.S., F.G.S.

12. *Half Hour Recreations in Popular Science.* (Estes & Lauriat, Boston.)—No. 11 of this series contains an article by Robert Hunt, F.R.S., on Coal as a Reservoir of Power, and another by Professor Clifford, on Atoms.

13. *On the so-called Land-Plants of the Lower Silurian of Ohio.*—On page 113 of this volume, six lines from the top, after the word exceptions, insert—and those of a very few foreign Lycopodiaceous plants: a clause inadvertently omitted. J. S. N.